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Exchange Rate Predictability†

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The main goal of this article is to provide an answer to the question: does anything forecast exchange rates, and if so, which variables? It is well known that exchange rate fluctuations are very difficult to predict using economic models, and that a random walk forecasts exchange rates better than any economic model (the Meese and Rogoff puzzle). However, the recent literature has identified a series of fundamentals/methodologies that claim to have resolved the puzzle. This article provides a critical review of the recent literature on exchange rate forecasting and illustrates the new methodologies and fundamentals that have been recently proposed in an up-to-date, thorough empirical analysis. Overall, our analysis of the literature and the data suggests that the answer to the question: "Are exchange rates predictable?" is, "It depends"—on the choice of predictor, forecast horizon, sample period, model, and forecast evaluation method. Predictability is most apparent when one or more of the following hold: the predictors are Taylor rule or net foreign assets, the model is linear, and a small number of parameters are estimated. The toughest benchmark is the random walk without drift. (*JEL* C53, F31, F37, E43, E52)

1. *Introduction*

The objective of this article is to offer a critical survey of the literature on predicting exchange rates in the last ten years. Since Meese and Rogoff (1983a, 1983b, 1988), it has been well known that exchange rates are very difficult to predict using economic models; in particular, a simple, *a*-theoretical model such as the random walk is frequently found to generate better exchange rate forecasts than economic models. The latter finding is known as "the Meese and Rogoff puzzle." It is important to note that Meese and Rogoff's (1983a, 1983b) finding that the random walk provides the best prediction of exchange rates should not be interpreted as a validation of the efficient market hypothesis. The efficient market hypothesis states that, in the absence of risk premia or when time variation in risk premia tends to be small relative to variation in fundamental pricing factors, bilateral exchange rates are the market's best guess of the relative, fundamental value of two currencies based on all available information at that time. The efficient market hypothesis does not mean that exchange rates are unrelated to economic fundamentals, nor that exchange rates should fluctuate

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randomly around their past values. Hence the puzzle. However, the recent literature has identified new macroeconomic and financial predictors that claim to forecast exchange rates. The goal of this article is to review both traditional as well as newly proposed exchange rate predictors and evaluate their ability to forecast exchange rates. The main goal is to provide an answer to the questions: Are exchange rates predictable? And, if so, which predictors are the most useful to forecast exchange rates?

When trying to answer these questions, a series of complications arise. First, a wide variety of predictors, models, estimation methods, measures of predictive content, and evaluation tests have been used in the literature. Thus, researchers attempting to forecast exchange rates need to make several choices, such as which predictors to use, which forecast horizon to predict, which model to estimate, which data frequency, and which sample. One of the goals of this paper is to provide guidance to researchers on navigating the existing literature, as well as to provide a reliable overview of established findings that can be helpful in making these choices. Second, existing papers rely on different predictors, tests, samples or databases; it is possible that such predictors might have lost their forecasting ability, or may not be robust to other databases or samples. In addition, while a predictor might be successful according to one metric/test, it may not be so according to a different one. We therefore perform a thorough empirical evaluation of the success of the predictors identified in the literature using the most recent techniques and databases. Thus, our article starts with a critical overview of existing predictors and the empirical stylized facts identified in the literature, with particular emphasis on the last ten years. Then, we illustrate the existing empirical evidence using the most up-to-date data and evaluation techniques in order to answer the question: does anything forecast exchange rates?

This article should be of interest for several audiences. Economists and researchers in academia will find that the literature review and the empirical investigation provide guidance to navigate the literature, and will be useful for their work. Practitioners and forecasters at central banks and private businesses will also be interested in knowing which predictors, models and methodologies successfully predict exchange rates. Policymakers, for whom successful policy decisions crucially depend on successful forecasts, should also be interested in our assessment on where the literature stands. Finally, newspapers' frequent discussions of exchange rate forecasting suggest that this literature review would be useful beyond academia and policy circles.

More in detail, why are exchange rate forecasts useful for central banks and policymakers? Wieland and Wolters (2013) provide a detailed review on how forecasts are used in policymaking. Typically, forecasts are used to project the consequences of particular policy measures for policymakers' targets. According to Greenspan (1994, p. 241), "implicit in any monetary policy action or inaction is an expectation of how the future will unfold, that is, a forecast." Wieland and Wolters (2013) provide empirical evidence that Central Bank policies in the United States and Europe are described by interest rate rules, where interest rates respond to forecasts of inflation and economic activity, rather than outcomes. Not only economic policy relies on macroeconomic forecasts: the path of the policy may directly affect the forecasts ("projections") of macroeconomic aggregates. In the United States, for example, prior to each Federal Open Market Committee meeting,[1] the Federal

[1] The Federal Open Market Committee is the meeting where U.S. monetary policy is decided.

Reserve staff produces forecasts of several macroeconomic aggregates at horizons up to two years as a basis for their discussions. The variables forecasted by the staff include exchange rates, which influence current account projections, as well as U.S. real GDP growth, eventually. As pointed out in Edge, Kiley, and Laforte (2010), among others, the Federal Reserve staff forecast is derived from data, a variety of models and forecasting techniques, as well as expert judgment. Typically, these forecasts are conditioned on a specific future time path for the federal funds rate, the main instrument of monetary policy. The policy scenarios considered by the Federal Reserve staff may also include dollar depreciation/appreciation scenarios (i.e., scenarios in which the dollar appreciates or depreciates more than in the baseline forecasts, where typically it is assumed to be constant, according to the random walk model). Policy decisions are then taken on the basis of what the policymaker deems the most likely scenario. Exchange rate projections are also especially important for central banks of countries that are heavy importers/exporters of commodities. For example, one of the models used at the Bank of Canada is Amano and van Norden's (1995), where real exchange rates depend on terms of trade (see Coletti and Murchison 2002).

At the same time, it should be noted that this review has an empirical, "reduced-form" focus. There are several reasons behind this choice. First, the majority of the empirical work in this area is done with a reduced-form approach; second, while there are theoretical structural models of exchange rate determination, typically they are too stylized to be literally taken to the data and successfully used for forecasting exchange rates. Moreover, fully developed structural models typically do not fit exchange rate data well, not to mention forecast them. Thus, while this article will sketch several "theoretical" models of exchange rate determination, this discussion is mainly provided to motivate the choice of economic predictors that have been considered in the literature. Furthermore, throughout the paper we focus on monthly and quarterly frequencies, as they are the ones of interest to economists; we will not consider very high frequency data analyses that are instead mostly of interest to risk management and finance. Finally, there is a large literature on in-sample estimation of exchange rate models. In-sample fit does not necessarily guarantee out-of-sample forecast success, as we will discuss. Thus, in this overview we will mainly focus on out-of-sample forecasts, although we will provide some discussion of in-sample fit. Note that, typically, real exchange rates are fitted in-sample, while nominal ones are forecasted out-of-sample; therefore, we will focus on the latter.[2]

Overall, our analysis of the literature and the data suggests that the answer to the question "Are exchange rates predictable?" is, "It depends." In fact, it depends on the choice of predictor, forecast horizon, sample period, model, and forecast evaluation method. Predictability is most apparent when one or more of the following hold: the predictors are Taylor rule and net foreign assets fundamentals, the model is linear, and a small number of parameters are estimated. The toughest benchmark is the random walk without drift. There is some instability over samples for all models, and there is no systematic pattern across models in terms of which horizons or which sample periods the models predict best. Among the negative findings on which the literature has reached a consensus, typically, purchasing power parity (PPP) and monetary models have no success at short (less than 2–3 years) horizons.

More in detail, we draw five general conclusions.

[2] See Rogoff (1996) for a review of in-sample fit of real exchange rate models.

First, the degree of success in forecasting exchange rates out-of-sample does depend on the choice of the predictor. Although there is disagreement in the literature, overall the empirical evidence is not favorable to traditional economic predictors (such as interest rates, prices, output and money).[3] Instead, Taylor-rule fundamentals and net foreign asset positions have promising out-of-sample forecasting ability for exchange rates. The consensus in the literature is that the latter fundamentals have more out-of-sample predictive content than traditional fundamentals; the disagreement in the literature is in the degree to which they can resolve the Meese and Rogoff puzzle.

Second, overall, among the model specifications considered in the literature, the most successful are linear ones.[4] Typically, in single-equation linear models, the predictor choice matters more than the model specification itself.[5]

Third, data transformations (such as detrending, filtering and seasonal adjustment) may substantially affect predictive ability, and may explain differences in results across studies.[6] Another important factor that, for some fundamentals, may affect predictive ability is the use of real-time rather than revised data.[7] For a given model and predictor, predictive ability seems also to depend on the choice of the country. On the other hand, the frequency of the data does not seem to affect predictability.

Fourth, empirical results vary with the benchmark model, the sample period, forecast evaluation method, and the forecast horizon. The random walk consistently provides the toughest benchmark. Different models vary in terms of which sample periods and forecast horizons work best, with no apparent overall pattern.

Finally, our empirical analysis confirms several findings in the literature: while several predictors display in-sample predictive ability for future exchange rates, only Taylor rules display consistently significant out-of-sample forecasting ability at short horizons; and panel monetary models display some forecasting ability at long horizons. Furthermore, our analysis reveals instabilities in the models' forecasting performance: the predictability of fundamentals varies not only across countries, models and predictors, but also over time. None of the predictors, models, or tests systematically finds empirical support of superior exchange rate forecasting ability across all countries and time periods: when predictability appears, typically it does so occasionally for some countries and for short periods of time. These findings lead to new challenges: Why does predictability change over time? Is it possible to design ways to exploit instabilities to improve exchange rates' forecasts? We discuss a few of these challenges in the last section.

There are several literature reviews on exchange rate predictability. How is this literature overview different from the existing ones? Frankel and Rose (1995) review the empirical literature on exchange rates up to 1995, whereas we focus on more recent contributions and include a thorough empirical analysis that includes recent data as well as predictors that have been identified in the last decade. Engel, Mark, and West (2007) focus on explaining the fluctuations of exchange rates using selected models, countries, and fundamentals; our analysis considers a broader set of fundamentals and

[3] Except possibly for monetary fundamentals at long horizons and interest rates at short horizons.

[4] Among them, error correction models (either single-equation or panel) are successful at long horizons, although there is disagreement among researchers regarding the degree of robustness of the result.

[5] For example, whether the researcher uses contemporaneous, realized or lagged fundamentals.

[6] For example, predictability of the monetary ECM model is much weaker or completely disappears after estimating the cointegrating parameters.

[7] This is a concern for monetary fundamentals but less of a concern for Taylor-rule fundamentals.

more recent data. Melvin, Prins, and Shand (2013) focus on forecasting exchange rates from a financial investor's point of view, e.g., carry trades; we focus instead on forecasting exchange rates using economic models and macroeconomic predictors. Lewis (1995) focuses on reviewing several puzzles in international financial markets, especially risk premia and home bias; we focus instead on assessing stylized facts on exchange rate predictability. Finally, Rogoff (1996) and Froot and Rogoff (1995) focus on PPP, according to which exchange rate fluctuations reflect fluctuations of countries' relative prices; while we do consider PPP as a predictor, our broader analysis includes several other predictors that have been considered in the literature to forecast exchange rates.[8]

The article is organized as follows. Section 2 provides a simple guiding example. Sections 3 to 6 review the literature: section 3 discusses the predictors that have been used to forecast nominal exchange rates; section 4 reviews a variety of models estimated in the literature; section 5 examines the characteristics of the data used in the literature; and section 6 overviews the forecast evaluation methods. At the end of each section, we provide a summary of the main findings in the literature. Finally, section 7 revisits the empirical evidence in an up-to-date, thorough empirical exercise.

2. *A Guiding Example*

Before reviewing in detail the predictors, models, data, and forecast evaluation methods proposed in the literature, let us consider a simple example to fix ideas about several basic concepts that we will discuss in this review. Readers that are familiar with basic in-sample fit and out-of-sample forecast methodologies can move directly to the next section.

Let the (log) of the exchange rate be denoted by s_t and let the (univariate) predictor (or fundamental) be denoted by f_t. Examples of predictors used in the literature are discussed in detail in the next section, and the choice of the data is discussed in section 5.

The relationship between the exchange rate and its fundamental can be described by several models (see section 4). For expositional purposes, let the model be linear and such that it does not include a constant term:

$$E_t(s_{t+h} - s_t) = \beta f_t, \qquad t = 1, 2, \ldots, T,$$

where T is the total size of the available sample and h is the forecast horizon.

The model's performance is typically evaluated relative to that of a benchmark model. Let the benchmark model be the random walk without drift:

$$E_t(s_{t+h} - s_t) = 0.$$

In fact, we will argue in section 6 that the random walk without drift is the appropriate benchmark for the analysis.

The predictive ability of the fundamental can be evaluated according to in-sample fit or out-of-sample forecast performance. In-sample fit is typically evaluated by estimating β over the full sample,

$$\hat{\beta}_T = \left(\sum_{t=1}^{T} f_t^2\right)^{-1} \left(\sum_{t=1}^{T} f_t(s_{t+h} - s_t)\right),$$

and calculating a t-test on β: if the fundamental contains relevant information, then β

[8] For shorter reviews on exchange rate predictability that focus on specific topics, see: Neely (1997) on technical trading rules; Bailliu and King (2005) on exchange rate models and the Canadian experience; Neely and Dey (2010) on the effects of macroeconomic news announcements on exchange rates; Chinn (2011) for a survey on macroeconomic models of exchange rate determination; and Neely and Sarno (2002) on the empirical performance of the monetary model.

should be different from zero. The latter is known as an in-sample (traditional) Granger-causality test. If the test rejects, it signals that the predictor contains useful information for explaining exchange rate fluctuations over the full sample. However, this does not necessarily mean that the predictor contains useful information to predict exchange rate fluctuations in real time. To assess the latter, it is common to turn to forecasting.

To evaluate the models' out-of-sample forecasting ability, the sample is split into two parts: the in-sample portion, consisting of observations from 1 to R, and the out-of-sample portion, of observations $R+h$ to $T+h$, of size $P \equiv T-R+1$. In the rolling window forecasting scheme, the parameter is reestimated over time using the most recent R observations, where R is known as the estimation window size:

$$\hat{\beta}_t = \left(\sum_{j=t-R+h+1}^{t} f_{j-h}^2\right)^{-1} \times \left(\sum_{j=t-R+h+1}^{t} f_{j-h}(s_j - s_{j-h})\right),$$
$$t = R, R+1, \ldots, T,$$

to obtain a sequence of P h-step-ahead out-of-sample forecast errors, $\varepsilon^f_{t+h|t} \equiv s_{t+h} - s_t - \hat{\beta}_t f_t$, $t = R, R+1, \ldots, T$. Note that the random walk forecast error is simply $\varepsilon^{rw}_{t+h|t} \equiv s_{t+h} - s_t$.

Under the rolling window forecast scheme, the model parameters are reestimated progressively over time. An alternative forecast scheme is the recursive one, where the model parameters are always reestimated using all the previous observations. That is, $\hat{\beta}_t = \left(\sum_{j=h+1}^{t} f_{j-h}^2\right)^{-1}\left(\sum_{j=h+1}^{t} f_{j-h}(s_j - s_{j-h})\right)$, $t = R, R+1, \ldots, T$.

The forecasting ability of the model is measured by a loss function; for example, a common choice is the Root Mean Squared Forecast Error (RMSFE), which will be the objective of our analysis unless otherwise noted:[9]

$$RMSFE_f \equiv \frac{1}{P}\sum_{t=R}^{T}(\varepsilon^f_{t+h|t})^2.$$

The model forecasts better than the random walk if $RMSFE_f < RMSFE_{rw} \equiv \frac{1}{P}\sum_{t=R}^{T}(\varepsilon^{rw}_{t+h|t})^2$. To judge whether the model forecasts significantly better, one typically tests whether $RMSFE_f - RMSFE_{rw}$ is equal to zero against the alternative that the difference is negative, i.e. using a t-test. Several methods to compute the standard errors and other available test statistics are discussed in section 6.

3. *Which Predictors to Use?*

This section reviews several economic predictors of exchange rates that have been used in the literature. It explains why, according to economic theory, they should forecast exchange rates, reviews papers proposing/using them, and summarizes their empirical findings.[10] A convenient summary of the various predictors used in the literature is provided in tables 1 and 2. Some empirical findings are contradictory: the next sections investigate in detail the reasons behind the differences among the papers.

3.1 *Traditional Predictors*

The traditional predictors used in the literature include interest rates, prices, money, and output differentials.

3.1.1 *Interest Rate Differentials*

Uncovered interest rate parity (UIRP) dates back to Fisher (1896)—see Dimand

[9] Other loss functions are sometimes used, such as the Mean Absolute Error, $MAE_f \equiv \frac{1}{P}\sum_{t=R}^{T}|\varepsilon^f_{t+h|t}|$.

[10] For a more detailed introduction to these models, see Obstfeld and Rogoff (1996) and Mark (2001).

(1999). Fisher (1896) provided a general analysis of how interest rates can be related to expected changes in the relative value of units of account or commodities: one of the examples he considered concerned international currencies, and has become known as UIRP. UIRP states that, in a world of perfect foresight[11] with a nominal bilateral exchange rate S_t, investors can buy $1/S_t$ units of foreign bonds using one unit of the home currency, where S_t denotes the price of foreign currency in terms of home currency. Let the foreign bond pay one unit plus the foreign interest rate between time t and $t+h$, i^*_{t+h}. At the end of the period, the foreign return can be converted back in home currency and equals $S_{t+h}[(1+i^*_{t+h})/S_t]$ in expectation. By arbitrage and in the absence of transaction costs, this return must be in expectation equal to the return of the home bond, $1+i_{t+h}$. That is, $(1+i^*_{t+h})\,E_t(S_{t+h}/S_t) = 1+i_{t+h}$, where $E_t(\cdot)$ denotes the expectation at time t. Finally, by taking logarithms and ignoring Jensen's inequality, the previous UIRP equation can be rewritten as:

$$(1) \quad E_t(s_{t+h} - s_t) = \alpha + \beta(i_{t+h} - i^*_{t+h}),$$

where $s_t \equiv \ln(S_t)$, $\alpha = 0$ and $\beta = 1$, and h is the horizon. Similarly, covered interest rate parity (CIRP) predicts exchange rates according to: $E_t(s_{t+h} - s_t) = \alpha + \beta(F_t - s_t)$, where F_t denotes the h-period ahead forward rate at time t.[12]

The empirical evidence is not favorable to UIRP. Meese and Rogoff (1988) use eq. (1) to forecast real exchange rates out-of-sample using real interest rate differentials, and compare its performance with the random walk, finding that the latter forecasts better. Similarly, Cheung, Chinn, and Pascual (2005) and Alquist and Chinn (2008) find that, although for some countries UIRP forecasts better than the random walk at long horizons, its performance is never significantly better. Slightly more positive findings have been reported by Clark and West (2006) at short-horizons, and Molodtsova and Papell (2009) for some countries.[13] In-sample empirical evidence is not favorable to UIRP either. The consensus is that, typically, estimates of (1) display a negative and significant slope, and a constant significantly different from zero; see Froot and Thaler (1990) for a survey.[14]

3.1.2 *Price and Inflation Differentials*

According to PPP, the real price of comparable commodity baskets in two countries should be the same. That is, the price level in the home country, converted to the currency of the foreign country by the nominal exchange rate, should equal the price level of the foreign country. Thus, a unit of currency in the home country will have the same purchasing power in the foreign country. The theory, due to Cassel (1918), was developed during the debate on the appropriate value of nominal exchange rates among major industrialized countries after the hyperinflations in World War I. Let the logarithm of the commodity price (CP) index in the home

[11] See Lewis (1995) for a discussion of UIRP without perfect foresight.

[12] CIRP states that the spread between forward and spot exchange rates equals the nominal interest differential between two countries and was developed by Keynes (1923).

[13] Molodtsova and Papell (2009) estimate UIRP with unrestricted coefficients (both constant and slope) as well as without a constant and with an estimated slope. In the latter case, they only find marginal evidence of predictive ability for Australia and Canada; in the former case, they find strong evidence in favor of Japan and Switzerland, as well as marginal evidence in favor of Australia and Canada.

[14] Possible explanations include: the presence of a time-varying risk premium (Fama 1984; Backus, Foresi, and Telmer 2001); estimation biases (Bekaert and Hodrick 2001); imprecise standard errors (Baillie and Bollerslev 2000, Rossi 2007); and small samples (Chinn and Meredith 2004, who find positive evidence in longer samples, and Chen and Tsang 2013, who pool information from the whole term structure).

and foreign countries be denoted by p_t and p_t^*, respectively. Then, PPP implies that:

$$(2) \qquad s_t = \alpha + \beta(p_t - p_t^*) + \varepsilon_t,$$

where $\alpha = 0$ and $\beta = 1$.

The out-of-sample empirical evidence is not favorable to PPP either: Cheung, Chinn, and Pascual (2005) find that, although PPP forecasts better than the random walk at the longest horizons, its performance is never significantly better; at shorter horizons, it is significantly worse than the random walk. Whether PPP holds in-sample is also debated. In particular, two stylized facts emerge from Rogoff (1996). First, nominal exchange rates tend toward purchasing power parity in the long run, although the speed of convergence is remarkably slow. Second, short run deviations from PPP are substantially large. As Rogoff (1996) notes, deviations from PPP can be attributed to transitory disturbances in the presence of nominal price stickiness; thus, they should be short-lived (i.e., one to two years), while, in the data, half-life deviations from PPP range between three to five years.[15] Rogoff (1996, p. 647) called this empirical inconsistency the PPP puzzle. Possible concerns and explanations include underestimation of the uncertainty around point estimates (Cheung and Lai 2000, Kilian and Zha 2002, Murray and Papell 2002, Gospodinov 2004, Lopez, Murray, and Papell 2005, and Rossi 2005a)[16] and heterogeneity in disaggregate data (Imbs et al. 2005).[17]

3.1.3 *Money and Output Differentials*

According to the monetary model of exchange rate determination, bilateral nominal exchange rate fluctuations should reflect movements in countries' relative money, output, interest rates and prices. The monetary model was first introduced by Frenkel (1976) and Mussa (1976), and builds on a simple small open economy model where real output is exogenous. Real money demand is viewed as a function of income and the interest rate; by using UIRP and PPP to substitute relative interest rates and prices as function of exchange rates, one obtains a relationship between exchange rates, money and output differentials. In greater detail, let m_t be the logarithm of nominal money, y_t be the logarithm of real output, and the horizon, h, equals 1. Then, real demand for money is modeled as:

$$m_t - p_t = -\eta i_{t+1} + \phi y_t.$$

Assuming that a similar equation holds for the foreign country ($m_t^* - p_t^* = -\eta i_{t+1}^* + \phi y_t^*$, where for simplicity of notation we assumed that the coefficients are symmetric and asterisks denote foreign country variables) and taking the difference between the two gives the relative money demand equation: $m_t - m_t^* - (p_t - p_t^*) = -\eta(i_{t+1} - i_{t+1}^*) + \phi(y_t - y_t^*)$. One approach (valid if prices and exchange rates are completely flexible) is to assume that PPP holds at every point in time, and substitute it in the relative money demand equation to get the "*flexible price version of the monetary model*":[18]

[15] The half-life measures how many time periods it takes for the effects of a shock to PPP to decrease by 50 percent.

[16] On the one hand, higher uncertainty implies that the PPP puzzle is even larger than previously thought; on the other hand, this partially reconciles PPP with models with sticky prices, since confidence intervals also include very short half-lives.

[17] Imbs et al. (2005) find that, when heterogeneity is taken into account, the estimated persistence of real exchange rates and the half-life falls dramatically. Using panel techniques, Taylor and Sarno (1998) find evidence in favor of PPP in the long-run.

[18] This was also referred to as the Frenkel–Bilson model; the coefficient on money differential is unity, due to first degree homogeneity of relative money supply. See Meese and Rogoff (1983a).

$$s_t = \eta(i_{t+1} - i^*_{t+1}) - \phi(y_t - y^*_t) + (m_t - m^*_t). \tag{3}$$

In the presence of slow price adjustment, either the relative price level or inflation differentials are included as regressors to obtain the *"sticky price version of the monetary model"*:

$$s_t = \eta(i_{t+1} - i^*_{t+1}) - \phi(y_t - y^*_t) + (m_t - m^*_t) + \zeta(p_t - p^*_t); \tag{4}$$

in this case, PPP holds in the long run, but does not hold in the short run.[19]

The empirical evidence on the monetary model is mixed. The in-sample evidence is somewhat positive,[20] while the out-of-sample evidence is less positive. On the one hand, Meese and Rogoff (1983a, 1983b) demonstrate that the random walk forecasts exchange rates out-of-sample better than any of the monetary models above, eqs. (3) and (4). Their finding has been confirmed by Chinn and Meese (1995) for short horizon (one-month- to one-year-ahead) forecasts,[21] by Cheung, Chinn, and Pascual (2005), who find that the monetary model does not predict well even at longer horizons (i.e., five years), and by Alquist and Chinn (2008).[22] Molodtsova and Papell (2009) also find very limited empirical evidence in favor of the model.[23] On the other hand, Mark (1995) finds strong and statistically significant evidence in favor of the monetary model at very long horizons (i.e., three to four years). The robustness of Mark's (1995) findings has, however, been questioned by Berkowitz and Giorgianni (2001), Kilian (1999), Groen (1999), Faust, Rogers, and Wright (2003), and Rossi (2005c). Overall, the former four papers find less evidence in favor of predictive ability even at long horizons, whereas the latter finds more positive results. The next sections will shed light on the causes of the disagreement across the empirical findings.

3.1.4 *Productivity Differentials*

More general monetary models that include additional predictors have been considered by several authors. For example, Cheung, Chinn, and Pascual (2005) consider a model where PPP does not hold even in the long run; instead, relative prices $(p_t - p^*_t)$ are expressed as a function of productivity differentials z_t following Balassa (1964) and Samuelson (1964):

$$s_t = \eta(i_{t+1} - i^*_{t+1}) - \phi(y_t - y^*_t) + \zeta_1(m_t - m^*_t) + \zeta_2 z_t + \varepsilon_t. \tag{5}$$

Note that in the case of (5), the nominal exchange rate may depend on real variables. In some studies, the real price of nontradeables is included instead of productivity differentials. Cheung, Chinn, and Pascual (2005) measure productivity differentials by labor productivity indices (real GDP per employee). They find that the model with

[19] Sometimes inflation differentials are used instead of prices (Cheung, Chinn, and Pascual 2005) and coefficients left unrestricted.

[20] MacDonald and Taylor (1993), Husted and MacDonald (1998), Groen (2000, 2002), and Mark and Sul (2001) find cointegration between exchange rates and monetary fundamentals, while Sarantis (1994) does not. Rossi (2006) rejects that the coefficients of the monetary model are both constant and equal to zero, suggesting time-varying predictive ability.

[21] At longer horizons, Chinn and Meese's (1995) results are slightly more positive, although statistically significant only for the Yen/U.S. dollar exchange rate among the five currencies they consider.

[22] Chinn and Meese (1995) consider eq. (3); Alquist and Chinn (2008) consider the sticky price monetary model (eq. 4) with differentials of money, real GDP, interest, and inflation rates.

[23] They find evidence only for two countries among the twelve they consider.

productivity differentials does not forecast better than the random walk.[24]

3.1.5 *Portfolio Balance*

Traditional portfolio balance models (Frankel 1982, Hooper and Morton 1982) include a measure of stock balances:

$$(6)\ s_t = \beta_0 + \beta_1(i_t - i_t^* - E_t(s_{t+1} - s_t)) + b_t - b_t^*,$$

where b_t is the stock of home assets held by home and b_t^* is the stock of foreign assets held by home, and the unobservable term $E_t(s_{t+1} - s_t)$ is approximated by zero. Several measures of balances have been used in the literature as broad proxies: cumulated trade balance differentials, cumulated current account balance differentials, and government debt. Meese and Rogoff (1983a, 1983b) find that, even after augmenting the monetary model by a measure of trade balance differentials, the model still does not forecast better than the random walk, a finding confirmed by Cheung, Chinn, and Pascual (2005).[25]

3.1.6 *Summary of Empirical Findings*

Overall, the empirical evidence based on the traditional predictors is not favorable to the economic models. While the out-of-sample forecasting ability of the economic predictors occasionally outperforms that of a random walk in some studies for some countries/time periods, it definitely does not systematically do so. More importantly, with a few exceptions, their predictive ability is not significantly better than that of a random walk at short horizons. The main exception is the work by Clark and West (2006) regarding the out-of-sample predictive ability of UIRP; the next sections will investigate the reasons why their finding is different from the rest of the literature. At longer horizons, there is more evidence of predictive ability in favor of the monetary model, although the finding is contentious. At the same time, some predictors (i.e., interest rate differentials) show significant in-sample fit, although with coefficient signs that are inconsistent with economic theory.

3.2 *Taylor Rule Fundamentals*

Engel and West (2005, 2006) and Molodtsova and Papell (2009) propose fundamentals based on a Taylor rule for monetary policy (Taylor 1993). Taylor (1993) formalizes the idea that the monetary authority sets the real interest rate as a function of how inflation differs from its target level (the higher the inflation, the more contractionary monetary policy will be) and also as a function of the output gap y_t^{gap} (if output is below its potential, monetary policy will be more expansionary). Taylor (1993) originally proposed the following specification: $r_{t+1} = \phi(\pi_t - \bar{\pi}) + \gamma y_t^{gap} + \bar{r}$, where π_t is the inflation rate, $\bar{\pi}$ is the target rate of inflation, y_t^{gap} is the output gap, $r_{t+1} \equiv i_{t+1} - \pi_{t+1}$ is the real interest rate (defined as the difference between the nominal interest rate, i_t, and the inflation rate, π_t), and $\bar{r}$ is the equilibrium real interest rate. Clearly, if one considers two economies, both of which set interest rates according to a Taylor rule, by UIRP their bilateral exchange rate will reflect their relative interest rates, and thus, as a consequence, their output gaps and their inflation levels. This basic idea is at the core of the Taylor rule fundamental model of exchange rates. We will discuss this model in detail following the approach of Molodtsova and Papell (2009).

[24] Wright (2008) also includes productivity differentials among his predictors. See Section 4 for a discussion.

[25] Cheung, Chinn, and Pascual (2005) estimate a model where exchange rate fluctuations are functions of the government debt relative to GDP, the real interest rate, the net foreign asset position, the (log of the) terms of trade, the (log) price level differential and the relative price of nontradeables.

Molodtsova and Papell (2009) amend the Taylor rule to take into account two empirical facts. First, in an open economy setting, as the central bank attempts to maintain the nominal exchange rate at its purchasing power parity level (Svensson 2000), monetary policy also depends on the real exchange rate, $q_t \equiv s_t - p_t + p_t^*$. Second, interest rate changes are sluggish since central banks prefer to avoid overachieving their target (as in Clarida, Gali, and Gertler 1998). By adding these features to the original Taylor rule, they obtain:

$$i_{t+1} = (1-\rho)(\mu + \lambda\pi_t + \gamma y_t^{gap} + \delta q_t) + \rho i_t + v_{t+1}$$

for all countries except the United States, for which $\delta = 0$, and v_{t+1} is the monetary policy shock. That is, using asterisks to denote foreign country variables:

$$i_{t+1}^* = (1-\rho^*)\left(\mu^* + \lambda^*\pi_t^* + \gamma^* y_t^{gap^*} + \delta^* q_t\right) + \rho^* i_t^* + v_{t+1}^*$$

$$i_{t+1} = (1-\rho)(\mu + \lambda\pi_t + \gamma y_t^{gap}) + \rho i_t + v_{t+1}.$$

By taking the difference of the two equations, using UIRP and redefining the coefficients, one obtains the specification in Molodtsova and Papell (2009):

$$E_t s_{t+1} - s_t = \tilde{\mu} + \tilde{\delta} q_t + \tilde{\lambda}^* \pi_t^* + \tilde{\gamma}^* y_t^{gap^*} + \tilde{\lambda}\pi_t + \tilde{\gamma} y_t^{gap} + \rho i_t - \rho^* i_t^*, \tag{7}$$

which they refer to as the "asymmetric" Taylor rule. They also consider imposing the coefficient to be the same and excluding the real exchange rate, a specification they refer to as the "symmetric, homogenous" Taylor rule, and typically lagged interest rates are not included:

$$E_t s_{t+1} - s_t = \tilde{\mu} + \tilde{\lambda}(\pi_t - \pi_t^*) + \tilde{\gamma}\left(y_t^{gap} - y_t^{gap^*}\right). \tag{8}$$

Both the in-sample and the out-of-sample empirical evidence are mostly favorable to Taylor-rule fundamentals, although with exceptions. Regarding the in-sample evidence, Chinn (2008) estimates the Taylor model in-sample and finds that the coefficient signs are not consistent with theory, and that the choice of the gap measure is not innocuous.[26] Regarding the out-of-sample evidence, Molodtsova and Papell (2009) show that eq. (8) forecasts exchange rates out-of-sample significantly better than the random walk for several countries, although the performance depends on the exact specification.[27] Molodtsova, Nikolsko-Rzhevskyy, and Papell (2011), Giacomini and Rossi (2010), and Rossi and Inoue (2012) also find strong empirical evidence in favor of Taylor-rule fundamentals. On the other hand, Rogoff and Stavrakeva (2008) find that the empirical evidence in favor of Taylor-rule fundamentals is not robust.

Taylor rules are generally deemed to be a good description of monetary policy in the past three decades, but monetary policy may have changed during the recent 2007 financial crisis. Molodtsova and Papell (forthcoming) study exchange rate forecasting during

[26] Chinn (2008) argues that this model of exchange rate fluctuations builds on UIRP: if the latter does not hold in the data, it is surprising that the former holds. Engel and West (2006) calibrate the model in-sample for real exchange rates and find a correlation of 0.5 between fitted and realized real exchange rates.

[27] E.g., which output gap measure is used and whether a constant is estimated. Across the various specifications they consider, typically, exchange rates of four to seven countries out of twelve are significantly predictable.

the financial crisis by including indicators of financial stress in the Taylor rule, such as the Libor-OIS/Euribor-OIS differential, the Bloomberg and OECD financial condition indices, and the TED spread differential. Adrian, Etula, and Shin (2011) use instead measures of liquidity such as funding liquidity aggregates of U.S. financial intermediaries measured by stocks of U.S. dollar financial commercial paper and overnight repos. Both of the latter papers find positive evidence.[28]

3.3 *External Imbalance Measures*

Gourinchas and Rey (2007) argue that not only the current account, but the whole dynamic process of net exports, foreign asset holdings, and return on the portfolio of net foreign assets are important predictors of exchange rates. When a country experiences a current account imbalance, the traditional intertemporal approach to the current account suggests that the country will need to run future trade surpluses to reduce this imbalance. Gourinchas and Rey (2007) argue instead that part of the adjustment can take place through a wealth transfer between that country and the rest of the world occurring via a depreciation of the value of its currency. Thus, they propose "net foreign assets" (NXA) as a potential predictor for future exchange rate fluctuations. NXA is the deviation from trend of a weighted combination of gross assets, gross liabilities, gross exports, and gross imports, and measures the approximate percentage increase in exports necessary to restore external balance, that is, to restore the long run equilibrium of net exports and net foreign asset ratios.

The empirical evidence is overall favorable to external imbalance measures. Gourinchas and Rey (2007) and Della Corte, Sarno, and Sestieri (2012) find that the net foreign asset model can predict (effective) exchange rates out-of-sample significantly better than the random walk at both long and short horizons.[29] Alquist and Chinn (2008) find that in some subsample the net foreign asset model forecasts (bilateral) exchange rates better than the random walk at short horizons for some countries; the results are, however, less favorable at longer horizons.[30]

3.4 *Commodity Prices and Other Predictors*

Chen and Rogoff (2003) focus attention on commodity prices as a potential new macroeconomic fundamental for exchange rates. They focus on commodity currencies, that is, exchange rates for countries where primary commodities constitute a significant share of exports (i.e., Australia, Canada, and New Zealand). Their main idea is that, typically, exchange rates are endogenously determined in equilibrium together with other macroeconomic variables, so it is difficult to predict exchange rate changes based on reduced-form models. However, if it were possible to identify an exogenous shock to exchange rates, then that would cleanly predict exchange rate fluctuations. Chen and Rogoff (2003) argue that commodity price changes act as essentially exogenous shocks for small open economies; the economies with a large share of exports in primary commodities will typically experience exchange rate appreciations when the price of their commodity exports increases. Measures of commodity prices used in the literature to

[28] E.g., Adrian, Etula, and Shin (2011) find positive evidence for almost all the advanced countries as well as half of the emerging countries they consider.

[29] Della Corte, Sarno, and Sestieri (2012) use bilateral external imbalance measures, as opposed to the global measure used in Gourinchas and Rey (2007), which summarizes the net foreign assets position of a country vis-à-vis the rest of the world. They construct the measure of bilateral NXA based on Lane and Milesi-Ferretti's (2007) database.

[30] Alquist and Chinn (2008) use a proxy for NFA position based on the end-of-year U.S. foreign asset and liability data from the Bureau of Economic Analysis (BEA) and interpolated using U.S. quarterly financial account data from IFS.

forecast exchange rate include commodity price indices (as in Chen and Rogoff 2003, Chen, Rogoff, and Rossi 2010, and Ferraro, Rogoff, and Rossi 2011) and oil prices (as in Bacchetta, van Wincoop, and Beutler 2010, and Rossi and Sekhposyan 2011). Chen and Rogoff (2003) find in-sample empirical evidence in favor of commodity prices as predictors of exchange rates; Chen, Rogoff, and Rossi (2010) find that commodity prices are not significant out-of-sample predictors of exchange rates in quarterly data, and Ferraro, Rogoff, and Rossi (2011) find that they are in daily data.[31]

3.5 *What Have We Learned?*

The literature has considered a wide variety of predictors. Table 1 summarizes the main predictors that have been used for out-of-sample exchange rate forecasting and table 2, panel A, provides an overview of which predictors have been used in the papers we discussed. Overall, the empirical evidence is not favorable to traditional economic predictors, except possibly for the monetary model at very long horizons and the UIRP at short horizons, although there is disagreement in the literature. Both Taylor-rule fundamentals and net foreign asset positions have promising out-of-sample forecasting ability for exchange rates, although some papers question the robustness of the results. The consensus in the literature is that these fundamentals have more out-of-sample predictive content than traditional fundamentals; the disagreement in the literature is in the degree to which these new fundamentals can explain the Meese and Rogoff puzzle.

[31] Additional predictors that have been considered mainly for in-sample fit of exchange rate models include order flows (defined as the difference between the number of buyer initiated transactions and the number of seller initiated transactions). See Rime, Sarno, and Sojli (2010) and Chinn and Moore (2011). As discussed in Andersen et al. (2003, p. 59), order flows have the potential drawback that we remain ignorant about the macroeconomic determinants of high-frequency order flows.

4. *Which Models To Choose?*

Several models have been used in the literature in the attempt to forecast exchange rates. Models can be divided into three representative groups: single-equation, multiple equations, and panel models. The models in each of these groups can be either linear or nonlinear, and may or may not allow for cointegration (i.e., by adding an error correction term) or time variation in the parameters. This section provides a guide to the various model specifications that have been used in the literature, and evaluates both how the choice of the model may affect predictive ability as well as explain some of the differences in findings across papers. Table 2, panel A, provides a convenient overview.

4.1 *Single-Equation, Linear Models*

Meese and Rogoff (1983a, 1988) focus on models where exchange rate fluctuations are explained by the simple single-equation model:

$$E_t(s_{t+h} - s_t) = \beta_0 + \beta_1' f_{t+h}, \tag{9}$$

where the future, realized values of the fundamental f_{t+h} are used. We refer to (9) as the "*single-equation, contemporaneous, realized fundamental model*." The actual, rather than the forecasted, value of the fundamentals is used as a predictor by Meese and Rogoff (1983a, 1983b, 1988) to make sure that the lack of predictability of exchange rates is not due to poor forecasts of the fundamentals. The parameters are estimated either by simple OLS or by GMM (to deal with the endogeneity of the predictors). Meese and Rogoff (1983b) calibrate the parameter in a grid to explore the robustness of their results to possible inconsistencies in the parameter estimates. Cheung, Chinn, and Pascual

TABLE 1
LITERATURE REVIEW: PREDICTORS AND ECONOMIC MODELS

Predictors (f_t)	Economic fundamentals	Mnemonics
$i_t - i_t^*$	Interest rate differentials	i
$F_t - s_t$	Forward discount	F
$p_t - p_t^*$	(log) price differentials	p
$\pi_t - \pi_t^*$	Inflation differentials	π
$y_t - y_t^*$	(log) Output differentials	y
$m_t - m_t^*$	(log) Money differentials	m
z_t	Productivity differentials	z
$b_t - b_t^*$	Asset differentials	b
$y_t^{gap} - y_t^{gap*}$	Output gap differentials	y^{gap}
nxa_t	Net foreign assets	nxa
CP_t	Commodity prices	CP

Model	f_t	Mnemonics
UIRP (CIRP)	$i_t - i_t^*, (F_t - s_t)$	i, F
PPP	$p_t - p_t^*$ or $\pi_t - \pi_t^*$	p, π
Monetary model with flexible prices (I)	$[(i_t - i_t^*), (y_t - y_t^*), (m_t - m_t^*)]'$	i, y, m
Monetary model with flexible prices (II) (or Frenkel–Bilson model)	$[(y_t - y_t^*), (m_t - m_t^*)]'$	y, m
Monetary model with sticky prices (I)	$[(i_t - i_t^*), (y_t - y_t^*), (m_t - m_t^*), (p_t - p_t^*)]'$	i, y, m, p
Monetary model with sticky prices (II) (or Dornbusch–Frankel model)	$[(i_t - i_t^*), (y_t - y_t^*), (m_t - m_t^*), (\pi_t - \pi_t^*)]'$	i, y, m, π
Model with productivity differentials (or Balassa–Samuelson (1964) model)	$[(i_t - i_t^*), (y_t - y_t^*), (m_t - m_t^*), z_t]'$	i, y, m, z
Portfolio balance model (or Hooper and Morton (1982) model)	$[(i_t - i_t^*), (b_t - b_t^*)]'$	i, b
Taylor rule model	$[(\pi_t - \pi_t^*), [(y_t^{gap} - y_t^{gap*})]'$	π, y^{gap}
Net foreign asset model	nxa_t	nxa
Commodity prices	CP_t	CP

Notes: The table reports the name of the model, the fundamental predictors used in the model ("f_t"), and the mnemonics used to refer to these fundamentals in Table 2.

(2005) forecast exchange rates using eq. (9) either by using calibrated parameter values (based on economic theory)[32] or by estimating eq. (9) via OLS (ignoring endogeneity issues). Similarly, Bacchetta, van Wincoop, and Beutler (2010) and Ferraro, Rogoff, and Rossi (2011), among others, estimate eq. (9).[33]

To evaluate actual, ex ante predictability of the fundamental, one might consider

[32] For example, for PPP, they forecast future $s_{t+h} - s_t$ simply by using $p_{t+h} - p_{t+h}^*$.

[33] Ferraro et al. (2011) argue that the fundamental that they use (the rate of growth of commodity prices) can be considered essentially exogenous; thus, they can consistently estimate the parameters by OLS.

TABLE 2
LITERATURE REVIEW: MODELS, PREDICTORS AND DATA

A. Models and predictors					B. Data		
Reference	Linear	Eq.	Details	Predictors	Freq.	Sample	Countries
				Single equation models			
Abhyankar, Sarno,and Valente (2005)	L	ECM	calibr. coeff.	m, y	M	1977:1–2000:12	CA,JP,UK
Alquist and Chinn (2008)	L	ECM	estim. coeff.	i; (m, y, π, i); nxa	Q	1970:1–2005:Q4	CA,UK,EU
Bacchetta, van Wincoop,and Beutler (2010)	L	contemp.	real. fund.	m, y, u, i, CP	M	1975:9–2008:9	UK,SWI,JP,GER,AU,CA,NZ
Berkowitz and Giorgianni (2001)	L	ECM	calibr. coint.	m, y	Q	1973Q2–1991Q4	CA,GER,SWI,JP
Chen, Rogoff, and Rossi (2010)	L	lag fund.		ΔCP	Q	varies by country	AU,CAN,NZ,CHI,SA
Cheung, Chinn, and Pascual (2005)	L	diff.; ECM	estim. coint.	i; p; (m, y, i, π); others (z, b)	Q	1973Q2–2000Q4	CAN,UK,GER,JP,SWI
Chinn (1991)	NL	levels,diff,ECM	non-parametric	m, y, i; π, wealth	Q	1974Q1–1988Q4	GER,JP
Chinn and Meese (1995)	L/NL	ECM/LWR	calibr. coeff.	m, y, i; others (π, TB, q)	M	1973:3–1990:11	GER,CA,UK,JP
Chinn and Moore (2011)	L	ECM	estim. coint.	order flow	M	1999:1–2001:1	EU,JP
Clark and West (2006)	L	lag fund.		i	M	varies by country	CAN,JP,SWI,UK
Della Corte, Sarno, and Sestieri (2012)	L	lag fund.		nxa	Q (real t)	1973Q1–2007Q4	CAN,GER,UK,JP
Diebold and Nason (1990)	NL	univ.	LWR	Δs	W	1973–87	10 countries
Engel and Hamilton (1990)	NL	MS		s, i	Q	1973Q3–1988Q1	GER,FR,UK
Engel (1994)	NL	MS		s	Q	1973–1991	18 countries
Engel, Mark, and West (2009)	L	ECM factor		factor from s	Q	1973–2007	17 countries
Faust, Rogers, and Wright (2003)	L	ECM	calibr. coint.	y, m; (m, y, π, i, b)	Q (real t)	1973Q2–1991Q4	GER,JP,CAN,SWI
Ferraro, Rogoff, and Rossi (2011)	L	lag/contemp.		ΔCP	D (real t)	1984:12–2010–11	CA,NOR,SA,AUS,CHI
Giacomini and Rossi (2010)	L	lag fund.		π, y^{gap}	M	1973:1–2008:1	12 countries
Gourinchas and Rey (2007)	L	lag fund.		nxa	Q	1973:Q1–2004Q1	US effective exch.
Groen (1999)	L	ECM		m, y	M	1973:4–1994:12	GER,FR,NET,CA
Kilian (1999)	L	ECM	calibr. coint.	m, y	Q	1973Q2–1991Q4	CAN,GER,SWI,JP
Mark (1995)	L	ECM	calibr. coint.	m, y	Q	1973Q2–1991Q4	CAN,GER,SWI,JP
Meese and Rogoff (1983a)	L	levels	OLS,GLS,IV	(m, y, i); others (π, TB, F)	M	1973:3–1981:6	UK,GER,JP,trade-w.
Meese and Rogoff (1983b)	L	contemp.	calib. coeff.	(m, y, i); others (π, TB)	M	1973:3–1981:6	UK,GER,JP,trade-w.
Meese and Rose (1991)	NL	lag/contemp.	LWR	(m, y, p); others (TB)	M	1974–1987	CA,GER,JP,UK
Molodtsova, Nikolsko-Rzhevskyy, and Papell (2011)	L	lag fund.		π, y^{gap}	Q (real t)	1991Q1–2007Q3	EU
Molodtsova and Papell (2009)	L	lag fund.		π, y^{gap}; m, y; i; p	M	varies by country	12 countries
Molodtsova and Papell (2012)	L	lag fund.		π, y^{gap}, FCI	Q (real t)	1992Q4–2012Q1	EU
Qi and Wu (2003)	NL	ECM	neural net.	m, y, i	M	1973:3–1997:7	JP,UK,CA,GER
Rapach and Wohar (2006)	NL	univ	ESTAR	s	M	1980:1-1984:12	UK,GER,FR,JP

(*Continued*)

TABLE 2 (*Continued*):
LITERATURE REVIEW: MODELS, PREDICTORS AND DATA

A. Models and predictors					B. Data		
Reference	Linear	Eq.	Details	Predictors	Freq.	Sample	Countries
				Single equation models			
Rime, Sarno, and Sojli (2010)	L	ECM		order flows	D	2004:2–2005:2	UK,EU,JP
Rogoff and Stavrakeva (2008)	L	ECM		(m, y); (π, y^{gap}); nxa	Q	1973:1–2005:4	18 countries
Rossi (2005c)	L	ECM		m, y	Q	1973Q3–1998Q2	GER,JP
Rossi (2006)	L/NL	lag fund.	TVP	Δy, Δm, i	M	1973:3–1998:12	CA,FR,GER,IT,JP
Rossi and Inoue (2012)	L	ECM		π, y^{gap}; i	M	1973:3–2008:1	10 countries
Rossi and Sekhposyan (2011)	L	lag fund.		Δm, Δu, Δy, Δi, ΔCP	M	1975:9–2008:9	SWI,UK,CA,JP,GER
Schinasi and Swamy (1989)	L	contemp.	TVP-AR	m, y, i, TB	M	1973:3–1980:3	UK,GER,JP
Wang and Wu (2009)	L	lag fund.	semi-param.	(π, y^{gap}, q);(m, y)	M	varies by country	12 countries
West, Edison, and Cho (1993)	L/NL	univ.	GARCH/NP	s	W	1973:3–1989:9	CA,FR,GER,JP,UK
Wolff (1987)	L	contemp.	TVP-AR	m, y, i, π	M	1973:3–1984:4	GER,JP,UK
				Multiple equation models			
Canova (1993)	L	TVP-VAR		i	W	1979–87	FR,SWI,GER,UK,JP
Clarida et al. (2003)	NL	VECM-MS		F	W	1979:1–1998:52	FR,GER,JP,UK
Clarida and Taylor (1997)	L	VECM		F	W	1977:1–1993:52	UK,GER,JP
Diebold, Gardeazabal, and Yilmaz (1994)	L	VECM		s	D	3/1/1980–1/28/1985	CAN,FR,GER,IT,JP,SWI,UK
Diebold, Hahn, and Tay (1999)	L	density		s	30 min.	1996:1–1996:12	GER,JP
MacDonald and Taylor (1993)	L	VECM		m, y, i	M	1976:1–1990:12	GER
Mizrach (1992)	NL	LWR		Δs	D	1986:1–1988:12	FR,IT,GER
Rapach and Wohar (2002)	L	VECM		(m, y); i	Y	115 years	14 countries
Wright (2008)	L	contemp/lag	BMA	p, y, i, z, m, SP, D, CA	M,Q (real t)	1973–2005	CA,GER,JP,UK
				Panel models			
Adrian, Etula, and Shin (2011)	L	lag. fund.		repo; comm.paper	M	1993:1–2007:12	23 countries
Carriero, Kapetanios, and Marcellino (2009)	L	large BVAR		s	M	1995:1–2008:4	33 countries
Cerra and Saxena (2010)	L	level,demean	ECM; diff.	m, y	Y (real t)	1960–2004	98 countries
Engel, Mark, and West (2007)	L	VECM	calibr. coeff.	(m, y, p); (π, y^{gap})	Q	1973–2005	18 countries
Groen (2005)	L	ECM	calibr. coint.	m, y	Q	1975Q1–2000Q4	CA,JP,GER
Mark and Sul (2001)	L	ECM		m, y; p	Q	1973Q1–1997Q1	19 countries
Rapach and Wohar (2004)	L	ECM		m, y	Q	1973Q1–1997Q1	18 countries

(*Continued*)

TABLE 2 (*Continued*):
FORECAST EVALUATION AND EMPIRICAL CONCLUSIONS

	C. Forecast evaluation					D. Successful predictive ability?
Reference	Benchmark	Estim.	*h*	Forec.	Eval. method	
			Single equation models			
Abhyankar, Sarno, and Valente (2005)	VAR		3–48 mo.	1991:1	utility	Yes
Alquist and Chinn (2008)	RW	roll	1,4,20 qrs.	1987Q2	CW	No (except UIRP at long h and NFA for some countries at short h)
Bacchetta, van Wincoop, and Beutler (2010)	RW	roll	1 mo.	varies	MSFE	No
Berkowitz and Giorgianni (2001)	RW		1–16 qrs.	1981Q4	R2, DMW	No
Chen, Rogoff, and Rossi (2010)	RW, AR	roll	1 q.		ENCNEW	Yes
Cheung, Chinn, and Pascual (2005)	RW	roll (42, 59)	1,4,20 qrs	1987Q2	MSE,sign	No
Chinn (1991)	RW	roll (48)	1, 4 qrs.	1986Q1		No
Chinn and Meese (1995)	RW		1,12,36 mo.	1983:01		No (except monetary at long h for some countries)
Chinn and Moore (2011)	RW		1–3–6 mo.	3 years	CW	Yes (but significant only for one country)
Clark and West (2006)	RW	roll (120)	1 mo.	varies	DMW,CW	Yes (for UIRP at short horizons)
Della Corte, Sarno,and Sestieri (2012)		roll (80)		1993Q1	utility-based	Yes (for NXA)
Diebold and Nason (1990)	RW		1–4 qrs. (iter.)	1986W1		No
Engel and Hamilton (1990)	RW		1–4 qrs. (iter.)	1984Q1	MSE	Yes
Engel (1994)	RW, F		1–12 qrs.	1986	DMW	Yes
Engel, Mark, and West (2009)	RW, Taylor		1–16 qrs.	1999		Yes, somewhat (at long h)
Faust, Rogers, and Wright (2003)	RW	roll (40)	1 day			No
Ferraro, Rogoff, and Rossi (2011)	RW, RWWD	roll	1 mo.	varies	DMW	Yes
Giacomini and Rossi (2010)	RW	roll (50)	1–24 qrs.	1983:2	GR,CW	Somewhat (it depends on time period)
Gourinchas and Rey (2007)	RW		1–48 mo.	1978Q2	CW	Yes
Groen (1999)	RW	rec	1 mo.	1981:10	DMW	No
Kilian (1999)	RW	rec	1–16 qrs.	1981Q4	R2,DMW	No
Mark (1995)	RW	rec	1,6,12 mo.	1981Q4	R2,DMW	Yes (only at long h)
Meese and Rogoff (1983a)	RW,VAR	roll(93)	1,6,12 mo.	1976:11	MSFE,MAE	No
Meese and Rogoff (1983b)	RW,VAR		48 mo.	1976:11	MSFE,MAE	No
Meese and Rose (1991)	RWWD	rec	1–4 qrs.	1984:1	MSFE	Yes
Molodtsova, Nikolsko-Rzhevskyy, and Papell (2011)	RW	roll(26)		1999Q4	CW	Yes
Molodtsova and Papell (2009)	RW	roll(120)	1 mo.	1982:3	CW	Yes for Taylor; no for UIRP and monetary

(*Continued*)

TABLE 2 (*Continued*):
FORECAST EVALUATION AND EMPIRICAL CONCLUSIONS

	C. Forecast evaluation					D. Successful predictive ability?
Reference	Benchmark	Estim.	*h*	Forec.	Eval. method	
			Single equation models			
Molodtsova and Papell (2012)	RW	roll (26)	1 q.	2007Q1	CW	Yes
Qi and Wu (2003)		roll (.75T)	1,6,12 mo. (iter)	1990:1	DMW	No
Rapach and Wohar (2006)			1 to 3 mo.		DMW, PIT	No
Rime, Sarno, and Sojli (2010)			1 day	2004:6	utility–based	Yes
Rogoff and Stavrakeva (2008)				varies	various	Mostly no
Rossi (2005c)			1–12 qrs.		DMW	Yes, based on GC–robust tests; somewhat, based on TVP models
Rossi (2006)	RW	roll,rec	1 mo.		DMW , ENCNEW	Yes (for some countries)
Rossi and Inoue(2012)	RW	roll	1–16 qrs.	varies		Yes
Rossi and Sekhposyan (2011)	RW	roll	1–12 mo.	varies	DMW	No
Schinasi and Swamy (1989)			15 mo.	1980:4	MSFE, MAE	Yes (but no statistical significance analysis)
Wang and Wu (2009)	RW	roll (200)	1–12 mo. (iter)		interval, GW	Yes
West, Edison and Cho (1993)					utility–based	Yes
Wolff (1987)	RW	rec	1–24 mo. (iter)	1981:7	MSFE, MAE	Somewhat (only for one country)
			Multiple equations models			
Canova (1993)	RW		1–52 w. (iter)	1986	Theil	Yes
Clarida et al. (2003)	RW		4–52 w.	1996:1	DMW	Yes
Clarida and Taylor (1997)	RW	rec	1–12 mo	1977:1	MSFE, MAE	Yes
Diebold, Gardeazabal, and Yilmaz (1994)			1–26 days			No
Diebold, Hahn, and Tay (1999)	absolute				PIT	n.a.
MacDonald and Taylor (1993)	RW	rec	1–12 mo. (iter)	1989–1990	MSFE	Yes
Mizrach (1992)	RW		5 days	1974:1	MSFE, MAE	No
Rapach and Wohar (2002)	RW	rec			ENCNEW	Somewhat (depends on country)
Wright (2008)	RW		3–12 mo.	1990	MSFE	Yes (although magnitude of success is marginal)
			Panel models			
Adrian, Etula, and Shin (2011)	RW, AR(1)	roll(48)	1–12 mo.	1997:1	CW	Yes for advanced countries, somewhat for emerging countries
Carriero, Kapetanios, and Marcellino (2009)	RW	roll(84)	1–12 mo.	2001:1	GW	Yes
Cerra and Saxena (2010)	RW, RWWD	rec	1–5 yrs.	1984	various	Yes

(*Continued*)

TABLE 2 (*Continued*):
FORECAST EVALUATION AND EMPIRICAL CONCLUSIONS

	C. Forecast evaluation					D. Successful predictive ability?
Reference	Benchmark	Estim.	h	Forec.	Eval. method	
			Panel models			
Engel, Mark, and West (2007)	RW	rec	1–16 qrs.	1983	CW	Yes (but not always)
Groen (2005)	RW	rec	1–16 (iter)	1989Q1	bootstrap	Yes (at long horizons)
Mark and Sul (2001)	RW	rec	1, 16 qrs.	1983Q2	MSFE, Theil	Yes
Rapach and Wohar (2004)	heter.	rec(50)	1–16 qrs.			n.a.

Notes: Panel A. "L" denotes linear model and "NL" denotes nonlinear model. Mnemonics for the predictors are as in table 1. If a paper considers several models at the same time, their predictors are separated by a semicolon; for example, a paper that considers both UIRP and the monetary model with interest rates will be characterized by: i,(m, π, y, i). In addition, "SP" denotes stock prices; "q" denotes the real exchange rate; "CP" denotes commodity prices (either oil price or the commodity price); "D" denotes the dividend yield; "CA" denotes the current account; "TB" denotes the trade balance; "s" is the lagged exchange rate; "FCI" denotes financial condition indices; "r" denotes the real interest rate; "u" denotes unemployment; a Δ before a variable denotes the first difference of that variable. Note that Cheung, Chinn, and Pascual (2005) also consider the model with productivity (m, y, i, z) as well as a model with net foreign assets (p, r, b, terms of trade, net foreign assets and the relative price of nontradeables). "Calibr. coeff." means calibrated coefficients; "estim.coeff." denotes estimated coefficients; "calibr. coint." means calibrated values for the cointegration vector; "estim. coint." denotes estimated cointegration vector; "diff." means that the model is estimated with variables in differences; "semi-param." means that the model is estimated with semiparametric methods; "contemp." denotes contemporaneous predictors; "lag fund." denotes lagged fundamentals; "real.fund." means that the model uses ex post, realized values of the contemporaneous fundamentals for prediction; "univ." denotes a univariate model (with possible lags of exchange rates and no fundamentals); "ECM" denotes error correction model; "NP" denotes nonparametric; "LWR" denotes locally weighted regression; "MS" denotes Markov switching model; "neural net" denotes neural network model. Canova's (1993) model includes stochastic volatility; in Cheung, Chinn, and Pascual (2005), the contemporaneous fundamental model is estimated with OLS whereas the cointegrating vector in the ECM model is estimated via the Johansen procedure; Wright (2008) also includes the ratio of the current account to output among the regressors; in Adrian, Etula, and Shin (2011), the repo- and commercial paper variables (here labeled "repo;comm.paper") are detrended over the full sample. Panel B. "Freq." denotes the frequency of the data: "Y" for yearly, "Q" for quarterly, "M" for monthly, "D" for daily, "W" for weekly, and "30min." for 30-minutes data. "real t" indicates that the study uses real-time vintages of data. "varies by country" means that typically the sample is 1973:3-2006:6, but for EU countries it may stop in 1998; "trade-w." denotes the trade-weighted exchange rate; "US effective exch." denotes the US effective exchange rate. Country mnemonics are as follows: "JP" is Japan, "GER" is Germany, "CA" is Canada, "UK" is the United Kingdom, "EU" is the Euro area, "SWI" is Switzerland, "AU" is Australia, "NZ" is New Zealand, "CHI" is Chile, "SA" is South Africa, "FR" is France, "NET" is the Netherlands, "IT" is Italy; "NOR" is Norway. Molodtsova, Nikolsko-Rzhevskyy, and Papell (2011) use quarterly data interpolated from annual data; Carriero, Kapetanios, and Marcellino (2009) use average exchange rates rather than end-of-period data. Panel C. The benchmark models include "Taylor" (the Taylor-rule model), the random walk ("RW"), the random walk with drift ("RWWD"), the forward discount ("F"), the autoregressive ("AR"), the vector autoregressive ("VAR"), and the heterogeneous coefficient panel model ("heter."). "Absolute" denotes cases in which there is no benchmark model and the forecast evaluation is based on absolute (rather than relative) terms. "Estim" denotes the parameter estimation method and includes rolling ("roll") and recursive ("rec") estimation; when available, the size of the rolling estimation window is included in parentheses; "T" denotes the total sample size. "h" is the forecast horizon; "1–16 qrs." typically denotes 1,4,8,12,16 quarters horizons; "(iter.)" denotes that forecasts at horizons bigger than one are made with an iterated method; otherwise it is assumed that the forecast method is direct. "Forec." denotes the starting date of the sample used for out-of- sample forecast evaluation (the end of the forecast sample is typically the end of the sample from table 3); "varies" means that various window sizes have been used so several out-of-sample periods have been investigated; Alquist and Chinn (2008) also consider 1999Q1–20005:Q4; Chinn and Meese (1995) also consider 1987–1990:11; Groen (1999) also considers 1981:10-1991:12; Meese and Rogoff (1983a) also consider 1978:11-1981:6; Mizrach (1992) also considers 1974:1-1979:3; Cheung, Chinn, and Pascual (2005) also consider post 1982; sample information for daily data is only approximate. "Eval.Method" denotes the method used for the forecast evaluation; "DMW" denotes the Diebold and Mariano (1995) and West (1996) test; "CW" denotes the Clark and West (2006) test; "ENCNEW" denotes the Clark and McCracken (2001)test; "GR" denotes the Giacomini and Rossi (2010) test; "Theil" denotes Theil's (1966) U statistic; "GW" denotes the Giacomini and White (2006) test; "sign" denotes the direction of change statistics; "utility-based" denotes utility-based statistics; "MSFE" denotes the Mean Square Forecast Error; "MAE" denotes the Mean Absolute Error; "R2" denotes the R-square statistic; "interval" denotes methods based on interval evaluation; "PIT" denotes the Probability Integral Transform; "various" indicates that several forecast evaluation methods have been used. Note that Rogoff and Stavrakeva (2008) consider all of the following statistics: CW, Theil, DMW, ENCNEW; Cerra and Saxena (2010) consider Theil, sign, DMW, CW. Panel D. It succinctly summarizes whether the author(s)' preferred specification beats the benchmark (i.e., model's predictive ability is successful).

the following models. The "*single-equation, contemporaneous, forecasted fundamental*" model is:

$$(10)\quad E_t(s_{t+h} - s_t) = \beta_0 + \beta_1' E_t f_{t+h},$$

where $E_t f_{t+h}$ is estimated based on information available up to time t, and the endogeneity of the fundamentals requires instrumental variable estimation. Eq. (10) has been considered by Meese and Rogoff (1983a) and Chinn and Meese (1995). In the "*single-equation, lagged fundamental model*":

$$(11)\quad E_t(s_{t+h} - s_t) = \beta_0 + \beta_1' f_t,$$

contemporaneous fundamentals are directly used for forecasting the h-step-ahead rate of growth of exchange rates.[34] Note that OLS can be used to estimate the parameters, since the regressors are lagged. Eq. (11) has been used by Wright (2008), Molodtsova and Papell (2009, forthcoming), Molodtsova, Nikolsko-Rzhevskyy, and Papell (2011), and Rossi (2005c), among others. A slight modification of eq. (11) is $E_t(s_{t+h} - s_t)/h = \beta_0 + \beta_1' f_t$, used by Gourinchas and Rey (2007) and Della Corte, Sarno, and Sestieri (2012).

The empirical evidence based on the single-equation linear model is mixed. Typically, contemporaneous, realized fundamentals are not successful.[35] Authors disagree on whether using forecasted fundamentals improves the empirical evidence in favor of the monetary model (Meese and Rogoff 1983a, find it doesn't, while Faust, Rogers, and Wright 2003 find that it does). The out-of-sample performance of lagged fundamentals depends on the predictor: it is poor for the monetary model with or without unemployment (see Rossi 2006, and Rossi and Sekhposyan 2011), but successful for the Taylor-rule model (Molodtsova and Papell 2009, forthcoming, Molodtsova, Nikolsko-Rzhevskyy, and Papell 2011, and Giacomini and Rossi 2010) and the net foreign asset model (Gourinchas and Rey 2007, and Della Corte, Sarno, and Sestierri 2012). Ferraro et al. (2011) find predictive ability using realized, contemporaneous commodity prices, though the predictive ability weakens considerably using lagged commodity prices. Overall, we conclude that, with the latter exception, it is the predictor that matters the most in determining the strength of the predictability rather than the exact specification of the single-equation linear model.

4.2 *Single-Equation, ECM models*

Since the work by Mark (1995), a model that has been widely used includes an error correction term. The Error Correction Model (ECM) assumes that there is a long-run relationship between the level of the exchange rate, s_t, and the level of the fundamentals, f_t; thus, in forecasting the rate of growth of the exchange rate, it includes a correction term that captures the long-run disequilibrium between the exchange rate and the fundamental levels:

$$(12)\quad E_t(s_{t+h} - s_t) \equiv \sum_{j=1}^{h} \Delta s_{t+j} = \beta_0 + \beta_1(s_t - \gamma' f_t).$$

To help intuition, consider the monetary model, eq. (3). Note that by substituting

[34] Note that the forecast is based on a parameter directly estimated from a regression of the current rate of growth of the exchange rate on the lagged value of the fundamental. See Marcellino, Stock and Watson (2006) for a review of direct and iterated forecast methods.

[35] See Meese and Rogoff (1983a, 1983b) for the monetary model; Cheung, Chinn, and Pascual (2005) for the monetary, productivity and net foreign assets measures; and Bacchetta, van Wincoop, and Beutler (2010) for the monetary model augmented by unemployment and oil prices.

the UIRP and PPP conditions in the relative money demand equation, we have: $m_t - m_t^* - s_t = -\eta(E_t s_{t+h} - s_t) + \phi(y_t - y_t^*)$, which leads to the popular approach of Mark (1995), where $E_t s_{t+h} - s_t = \alpha + \beta[(m_t - m_t^*) - \phi(y_t - y_t^*) - s_t]$. As long as $\beta < 1$, exchange rates revert back to their fundamental value $f_t \equiv (m_t - m_t^*) - \phi(y_t - y_t^*)$ over time. Thus, predictive ability should be stronger at longer horizons. The cointegration vector parameter (γ) can be calibrated—as in Mark (1995),[36] Chinn and Meese (1995), Abhyankar, Sarno, and Valente (2005), Berkowitz and Giorgianni (2001), and Kilian (1999)—or estimated (typically by Stock and Watson's (1993) DOLS), either over the full sample or recursively—as in Alquist and Chinn (2008), Chinn and Moore (2011), and Cheung, Chinn, and Pascual (2005).[37]

Positive evidence in favor of the ECM model at long horizons has been found by Mark (1995), whereas Cheung, Chinn, and Pascual (2005) and Alquist and Chinn (2008) find no predictive ability. Note that the former calibrates the cointegration parameters, whereas the latter estimate them. On the other hand, using exactly the same ECM specification, Kilian (1999) and Groen (1999) find no predictive ability for the monetary model at long horizons, whereas Rossi (2005c) does. We will investigate in detail the reasons behind the disagreement over the predictive ability of the monetary model at long horizons in Sections 5.1, 6.4, and 6.5.[38]

4.3 *Nonlinear Models*

Most of the literature focuses on linear models. Only a few papers focus on nonlinear models, including: nonparametric methods (locally-weighted regressions), as in Diebold and Nason (1990), Meese and Rose (1991), Chinn (1991), Mizrach (1992), and Chinn and Meese (1995); neural networks, as in Qi and Wu (2003); or (exponential) transition autoregressive models (ESTAR), as in Rapach and Wohar (2006). Again, the literature differs regarding whether actual, realized fundamentals or forecasted fundamentals are used—see table 2.

The empirical evidence is not favorable to nonlinear models. The vast majority of the literature finds that nonlinear models forecast poorly,[39] with some exceptions.[40] This finding is consistent with the more general finding that nonlinear models fit well in-sample,[41] but fail in out-of-sample forecasting exercises (Terasvirta 2006).

4.4 *Time-Varying Parameter (TVP) Models*

A special form of nonlinearity may be induced by time-variation in the parameters. There are several ways to deal with time variation in the parameters: either estimate a parametric model where the parameters change over time according to a rule (as in the Kalman filter approach by Wolff 1987, and Schinasi and Swami 1987; the Bayesian TVP model in Canova 1993; Stock and Watson's (1998) random walk coefficient model; or the Markov switching

[36] Mark (1995) calibrates $\gamma \equiv [1, -\phi] = [1, -1]$.

[37] The parameters β_0, β_1 are estimated by OLS.

[38] Amano and van Norden (1993) also use ECMs to predict the Canadian–U.S. dollar real exchange rate using world commodity price indices and interest rates; and Amano and van Norden (1995, 1998a, 1998b) study their cointegration properties.

[39] Including Chinn (1991) and Chinn and Meese (1995) for the monetary model; Diebold and Nason (1990) for univariate models; Mizrach (1992) for locally weighted regression model across several currencies; Qi and Wu (2003); and Rapach and Wohar (2006).

[40] Meese and Rose (1991) find significant in-sample and out-of-sample predictability in the nonlinear monetary model. Satchell and Timmermann (1995) show that, although the squared forecast errors of nonlinear models are higher than those of the random walk, nonlinear models correctly predict a large proportion of the sign of exchange rate changes for several countries.

[41] Taylor and Peel (2000) and Kilian and Taylor (2003) are examples of in-sample estimation of nonlinear models and out-of-sample forecasting, respectively, in the case of real exchange rate models.

model of Engel and Hamilton (1990) and Engel (1994)) or take averages across models, either via a Bayesian model averaging (BMA) approach (Wright 2008) or forecast combinations (Timmermann 2006).[42]

The "*single-equation time-varying parameter*" model used in Schinasi and Swamy (1989) and Wolff (1987) is:

$$s_t = \beta_t' f_t + u_t, \tag{13}$$

$$\beta_t = G\beta_{t-1} + K + Av_t,$$

where the parameter β_t changes over time according to an autoregressive process, K and A are constants, and u_t and v_t are unforecastable shocks.[43] The "*random walk coefficient time-varying parameter*" model (Stock and Watson 1998) imposes $G = I$ and $K = 0$. In the "*Markov switching*" model, the relationship between exchange rates and fundamentals depends on an unobservable variable. An example of the Markov switching model is: $([s_t, f_t']' | R_t) \sim N(\mu_{R_t}; \Omega_{R_t})$, where $R_t = \{R_1, R_2, \ldots, R_k\}$ is an unobservable regime (or state). For example, for $k = 2$, R_1 could be a recession and R_2 could be an expansion; in this case, the relationship between exchange rates and fundamentals depends on the state of the business cycle. The regime evolves stochastically, according to a law of motion (see Engel and Hamilton 1990, for details on the estimation and the model).

The empirical evidence is mixed. Some papers find out-of-sample forecast improvements over the random walk—see Schinasi and Swamy (1989) and Wolff (1987) for TVP models,[44] and Engel and Hamilton (1990) and Engel (1994) for a Markov switching univariate model. In other cases, the forecast improvements are unclear: Rossi (2006) finds that a random walk coefficient TVP model outperforms the random walk for only one country; Bacchetta, van Wincoop, and Beutler (2010) instead conclude that time variation cannot explain the Meese and Rogoff puzzle.[45]

4.5 *Multivariate Models*

Many different types of multivariate models have been used in the literature, typically generalizations of the single-equation models discussed above. Let $Y_t \equiv ([s_t, f_t']')$. Among the multivariate models, the following have been prominent.

4.5.1 *VARs*

VARs have been considered in Meese and Rogoff (1983a, 1983b):

$$A(L)Y_t = u_t, \tag{14}$$

where $A(L) = I - A_1 L - \cdots - A_p L^p$ and L is the lag operator. A special version of VARs are Bayesian VARs (BVARs), that is, VARs estimated with a large number of variables imposing some Bayesian shrinkage for the parameters, which otherwise would be very imprecisely estimated in the small samples typically available to researchers. Meese and Rogoff (1983a, 1983b) find that VARs with monetary fundamentals do not improve over the random walk. The BVARs with a large number of exchange rates in Carriero, Kapetanios, and Marcellino (2009) outperform the random walk even at short

[42] The difference between BMA and forecast combinations is that BMA estimates the weights in the forecast combination using Bayesian methods, whereas forecast combinations typically use equal weights or weights estimated with frequentist methods.

[43] Wolff (1987) estimated the TVP model using the Kalman filter.

[44] Although the former did not test their significance and the latter finds significant evidence for one country out of three. Cheung and Erlandsson (2005) find in-sample empirical evidence in favor of a Markov switching model for exchange rates.

[45] Although Chinn (2010) and Giannone (2010) debate that conclusion.

horizons. Note that the BVAR considered by Carriero, Kapetanios, and Marcellino (2009) does not include any economic fundamentals and, therefore, does not shed light on which fundamentals economists should focus on.

4.5.2 *Factor Models*

The *"factor ECM model"* proposed by Engel, Mark, and West (2009) is:

$$(15)\quad E_t(s_{i,t+h} - s_{i,t}) \;=\; \beta_i \;+\; \beta_h(\delta_i' \varphi_t - s_{i,t}),$$

where φ_t is the factor extracted from the explanatory variables X_t, ($X_t = \lambda\varphi_t + u_{i,t}$, where the explanatory variables are a panel of exchange rates), i denotes the country, and the number of countries considered is large.[46] Engel, Mark, and West (2009) find that in some cases, the factor model improves forecasts at long horizons but does not improve short-horizon forecasts.[47]

4.5.3 *VECMs*

The single-equation ECM model, eq. (12), is a simplification of the traditional *"multi-equation VECM model"*:

$$(16)\quad \Delta s_{t+1} = \beta_0 \;+\; \beta_1(s_t - \gamma' f_t) \;+\; \beta_2(L)\;\Delta s_{t-1} \;+\; \beta_3(L)\;\Delta f_{t-1}$$

where the short run dynamics is eliminated, and $\Delta \equiv 1 - L$. The empirical evidence on VECMs is mixed: some papers find positive evidence (i.e., MacDonald and Taylor 1993, for the monetary model; Clarida and Taylor 1997, for forward rates) while others are more negative (Rapach and Wohar 2002, and Diebold, Gardeazabal, and Yilmaz 1994).[48] Overall, the literature suggests that single-equation ECM specifications are preferable to VECMs because the short-run dynamics of exchange rates is difficult to estimate (cfr. Cheung, Chinn, and Pascual 2005, p.1156).

4.5.4 *Multivariate Time-Varying Parameter Models*

Multivariate models can also have time-varying parameters: a multivariate version of the time-varying parameter model (13) has been used in Canova (1993).[49] Due to their complexity, these models are estimated by Bayesian methods. The empirical evidence shows that multivariate TVP models may provide forecast improvements over the random walk (see Canova 1993, for the multivariate TVP model with interest rates, and Clarida et al. 2003, for the Markov Switching VECM model with forward rates).

4.5.5 *Bayesian Model Averaging (BMA)*

An alternative way to exploit predictability from many regressors is via Bayesian model averaging, which combines forecasts from competing models using weights that are estimated by posterior probabilities. Wright (2008) considers BMA with predictors that include traditional predictors as well as stock prices, dividend yields, and the current account. He finds that, for most currencies, BMA with sufficiently high shrinkage produces forecasts that are better than the random walk, although the magnitude of the improvements is marginal.

4.6 *Panel Models*

Several panel models have been estimated in the literature. The *"panel ECM"*

[46] Eq. (15) may include additional control variables, such as the deviation of Taylor rules, monetary, or PPP fundamentals from the current exchange rate of the country.

[47] They also consider the alternative specification $E_t\, s_{i,t+h} = \beta_i'\, E_t\, \varphi_{t+h}$.

[48] Diebold, Gardeazabal, and Yilmaz (1994) reconcile their negative finding with the fact that powerful cointegration tests reject cointegration. See also Baillie and Bollerslev (1989, 1994) for in-sample tests of cointegration among exchange rates.

[49] In greater detail, Canova's (1993) model is: $B_t(L)\,Y_t = u_t$, where $u_t|\Im_t \sim N(0, V)$, $\Im_t$ is the information set, and $B_t = GB_{t-1} + K + Av_t$, where B_t is the vector containing all the parameters in $B_t(L)$ and $v_t \sim (0, \Sigma_t)$.

considered by Mark and Sul (2001), Groen (2005), Engel, Mark, and West (2007), Cerra and Saxena (2010), and Rapach and Wohar (2004) is:

$$E_t(s_{i,t+h} - s_{i,t}) \;=\; \beta_h(f_{i,t} - \gamma s_{i,t}),$$

where the error term contains an individual, time-invariant component, an aggregate, time variant component, and an individual, time variant component.[50] Mark and Sul (2001), Groen (2005), Engel, Mark, and West (2007), and Cerra and Saxena (2010) impose a known cointegrating parameter γ.[51] Rapach and Wohar (2004) estimate the cointegrating parameters recursively, but unfortunately do not compare the models' forecasts with the random walk. The "*panel, contemporaneous realized fundamental model*" used by Cerra and Saxena (2010) is: $E_t\,\Delta s_{i,t+h} = \beta f_{i,t+h}$, where $f_{i,t+h}$ is the actual, realized value of the fundamental. The "*panel, lagged fundamental model*" used by Adrian, Etula, and Shin (2011) is: $E_t\,\Delta s_{i,t+h} = \beta f_{i,t}$, where $f_{i,t}$ is the lagged value of the fundamental.

The empirical evidence suggests that panel ECMs are quite successful for the monetary model (see Mark and Sul 2001; Groen 2005; Engel, Mark, and West 2007; and Cerra and Saxena 2010) and for funding liquidity fundamentals (Adrian, Etula, and Shin 2011). However, they are less successful for PPP fundamentals (Mark and Sul 2001).

4.7 *What Have We Learned?*

Among the model specifications considered in the literature, the least successful are nonlinear specifications and the most successful are linear specifications. Among the linear models, the single-equation ECM and the panel ECM models are the most successful at long horizons, although there is disagreement among researchers about the degree of robustness of the results. Typically, but not always, for single-equation linear models the predictor choice matters more than whether the researcher uses contemporaneous, realized or lagged fundamentals; the linear monetary model does not perform well at any horizons, whereas Taylor-rule fundamentals and net foreign assets are successful predictors at short-horizons. Models with time-varying parameters show some degree of success, although most of the favorable empirical evidence is based on studies in the late 1980s through the early 1990s, and the most recent analyses report less success (although with slightly different specifications). Among the multivariate models, the most successful specification is the panel ECM, although there is some evidence in favor of BVARs, BMA, and factor ECM models.

The next section will investigate why, among researchers using the same model, there is substantial disagreement across empirical findings by examining in detail other important dimensions in which the studies differ.

5. *Which Data To Use?*

Existing studies in the literature differ considerably depending on the characteristics of their data. In particular, they differ depending on the data transformation they perform, the countries they study, the sample they use, the frequency of the data, and whether the data are fully revised or real-time. This section reviews what choices have been made in the literature and what we know about how they potentially affect predictability; in particular, we include a discussion that revisits the empirical findings in the previous sections in this light. Panel B

[50] Its forecasts are: $\beta_h(f_{i,t} - \gamma s_{i,t}) + \zeta_i + \frac{1}{t}\sum_{j=1}^{t}\theta_j$, where ζ_i is the individual, time-invariant component and θ_t is the aggregate, time-variant component.

[51] In particular, Groen (2005) imposes knowledge on the number of cointegrating vectors as well as the parameters of the common cointegrating vector, and only reestimates the intercept and the coefficients of short-run dynamics.

in table 2 overviews data choices in selected papers in the literature.

5.1 *End-of-Sample versus Filtered Data and Calibrated Parameters*

When conducting out-of-sample forecast evaluation, it is important to make sure that the information contained in future data is not trivially used to estimate the models' parameters. To clarify, consider a forecaster estimating the model at time t and producing the h-step-ahead forecast for time $t + h$. It is important that when the forecaster produces the forecast at time t, the information contained between times t and $t + h$ (or later) is not used directly or indirectly in the estimation; otherwise, the model has an unfair advantage relative to the random walk. In other words, the model would be given the advantage of peeking at data after time t, which would not have been available to the forecaster in real time. One example when this might happen is when the data are detrended over the full sample or detrended by any two-sided filtering procedure. For example, when using the output gap to forecast exchange rates, it is important that the output gap, usually defined as the difference between output and a linear or a quadratic trend, be detrended using only information available at time t. Seasonal adjustment is another example where the treatment of the data is important. All the raw data used in Meese and Rogoff's (1983a) study were seasonally unadjusted: they performed the seasonal adjustment procedure themselves. The reasons are twofold: using raw data allows researchers to seasonally adjust the data in a consistent way across different series; furthermore, it allows them to avoid using information that was not available at the time that the forecast was made (for example, it is preferable to use a one-sided moving average seasonal adjustment filter, as opposed to a two-sided one, as forecasts based on two-sided filtered data implicitly use information which would have not been available at the time of the forecast). As a third example, one should focus on forecasting actual exchange rates, and not Hodrick–Prescott filtered exchange rates. As a fourth example, using calibrated fundamentals (e.g., linear combinations of macroeconomic predictors, where the coefficients are calibrated rather than estimated) is another example of "ad hoc" filtering, which could potentially give the model an unfair advantage since it is unclear how the parameters are calibrated (in particular, what data inspired the calibrated parameters). As a fifth example, sometimes, auxiliary parameters are imposed in an "ad hoc" manner, and it is unclear whether results are robust to such choices. Consider the case of a researcher using BMA techniques and shrinking the parameter towards the random walk (that is, imposing a rule to make the parameter smaller); if the rule is ad-hoc, it is unclear whether peeking at the whole dataset might have influenced the degree of shrinkage. Other examples (including the choice of the estimation sample or the use of real-time data) are discussed in detail below.

Let us critically overview how these ad hoc procedures may affect the favorable findings of predictive ability reviewed in the previous sections. Typically, in the ECM estimation literature, researchers impose a known cointegrating parameter.[52] The predictability found in VECMs with monetary fundamentals also typically relies on the cointegrating parameter estimated over the full sample:[53] Cheung, Chinn, and Pascual (2005) and Alquist and Chinn (2008) show that the

[52] For example, Mark (1995) and Chinn and Meese (1995). Typically, papers in this literature also impose knowledge on the number of cointegrating vectors.

[53] For example, MacDonald and Taylor (1994) find that the sticky-price monetary model estimated via cointegration outperforms the random walk out-of-sample; however, the cointegrating vector is estimated over the entire sample, generating forecasts that incorporate future realized values, and hence they are not truly ex ante.

empirical evidence in favor of long horizon predictability of the monetary model is much weaker or completely disappears after estimating the cointegrating parameters. Thus, some of the disagreements over the empirical performance of the monetary model at long horizons can be explained by differences in models' specification: the model performs better by imposing an ad hoc calibrated cointegration parameter than by estimating it.[54] In some panel analyses (i.e., Adrian, Etula, and Shin 2011) the fundamentals are detrended over the full sample prior to forecasting as well. The favorable evidence for the BVAR relies on choosing the degree of Bayesian shrinkage, and it is unclear how different degrees of shrinkage may affect the results.

5.2 *Forecasted versus Realized (or ex post) Fundamentals*

The previous section highlights that the same model can be estimated either using forecasted or realized fundamentals. The latter also give an unfair advantage to the fundamentals-based economic model. However, it is important to note that the original Meese and Rogoff (1983a, 1983b) analysis used future realized fundamentals, as opposed to fundamentals known at the time of the forecast, to predict exchange rates because they were proving a negative result. The result that Meese and Rogoff (1983a, 1983b) wanted to prove is that no fundamental forecasts exchange rates better than a random walk; by showing that not even realized, h-period-ahead fundamentals were capable of predicting exchange rates, they made their findings really stark. In other words, if forecasted h-period-ahead fundamentals had no predictive content for h-period-ahead exchange rates, that might be due either to the lack of predictive content or to the fact that forecasts of fundamentals are poor. By demonstrating that even when using realized fundamentals it is not possible to improve exchange rate forecasts, Meese and Rogoff (1983a, 1983b) were able to prove that the lack of predictive content in the fundamentals is really the cause of the problem. Table 2 provides an overview on the use of forecasted versus realized fundamentals in the literature. As noted in the previous section, typically for single-equation linear models, the predictor choice matters more than whether the researcher uses contemporaneous, realized or lagged fundamentals. For panel models, Cerra and Saxena (2010) demonstrate that the use of realized versus lagged fundamentals does not matter either.

5.3 *Countries and Samples*

Existing studies in the literature differ with respect to the countries they consider as well as the sample. Most studies focus on bilateral exchange rates versus the U.S. dollar, although there are exceptions.[55] Typically, the sample starts in 1973 to avoid the period of fixed exchange rates and typically, the end of the sample is constrained by data availability; for example, individual European countries' exchange rates are available only prior to the Euro unification. More recent studies have replaced the exchange rate of the countries that are now part of the European Union with the euro–dollar exchange rate; its value can be backcasted to the period before the unification as well, although the effects of backcasting are unclear. A clear message in the literature is that models and predictors that forecast exchange rates well in one country do not necessarily provide competitive forecasts for other countries (see Cheung, Chinn, and Pascual 2005).

[54] In fact, Groen (1999) argues that the cointegration relationship between exchange rates and monetary fundamentals is unstable and disappeared in the later sample.

[55] For example, Meese and Rogoff (1983a, 1983b) consider also the trade-weighted exchange rate.

5.4 *Frequency*

Existing studies in the literature also differ with respect to the frequency of the data they consider, ranging from low frequency, yearly data, to quarterly, monthly, and even weekly, daily, or very high-frequency data (such as those available at 30-minute intervals). There is clearly a trade-off between the frequency of the data and the span of the data, as lower frequency data (which are perhaps more informative regarding long-run trends in the data) are by construction available only for shorter samples.[56] Typically, studies interested in the long-term forecastability of exchange rates focus on quarterly data (i.e., Mark 1995). There are no striking discrepancies between empirical results of studies that focus on monthly data relative to those that focus on quarterly data; some of the studies that focus on daily data (for example, studies that use macroeconomic news announcements as predictors)[57] do generally find positive evidence for the model, but typically, they are estimated in-sample, so the real difference seems to be whether the models are compared in-sample versus out-of-sample. We conclude that the frequency of the data does not typically affect findings of predictive ability.[58]

5.5 *Data Revisions*

Typically, researchers evaluate the out-of-sample forecasting ability of their models using data available at the time of their study. However, the data that a researcher has available in 2000Q1 for, say, GDP in 1998Q1 is not the same as that available to a researcher in 1998Q1. The reason is that data are continuously revised by statistical agencies. The revisions include those due to statistical agencies acquiring additional source information to update their initial estimates, changes in the aggregation methods (fixed to chain weighting), changes in base years used for calculating real variables, or changes in definitions of the concept being measured. Financial data, such as interest rates and exchange rates, are never revised, but GDP, money aggregates, and inflation are. A real-time dataset collects vintages of data that were actually available to researchers at each point in time, before data revisions were applied to the data to obtain the finally revised data available at the end of the sample (or current vintage data). The conventional wisdom is that, for example, the use of revised data may lead researchers to include regressors that had little predictive content in real time and may exaggerate the forecasting ability relative to predictors that were actually available at the time (see Koenig, Dolmas, and Piger 2003, and Croushore and Stark 2001, 2003).[59]

The empirical evidence shows that the findings in favor of the long-horizon predictive ability of the monetary model are not robust to using real-time data. In particular, Faust, Rogers, and Wright (2003) show

[56] For example, longer spans of data have been considered by Rapach and Wohar (2002) at the price of restricting attention to yearly frequencies and a smaller sample. See also Taylor (2002) on using longer spans of data for analyzing real exchange rates.

[57] Macroeconomic news announcements are defined as the difference between expected and realized (announced) macroeconomic fundamental values. Expectations are typically measured by surveys. For example, Andersen et al. (2003), Ehrmann and Fratzscher (2005), Faust et al. (2007), and Fratzscher (2009) study whether, in-sample, exchange rates react in a very short window around the announcement.

[58] One exception might seem Ferraro, Rogoff, and Rossi (2011). However, even in their case, the main reason for the predictive ability is not just the use of daily data, but the use of both daily data and commodity prices as fundamentals.

[59] Note that the dependent variable (the exchange rate) is a financial variable and is never revised. Thus, unlike forecasting output growth, where the researcher needs to determine whether to forecast the first revision, the second revision, or the latest revised data, which value to forecast is an irrelevant issue when forecasting exchange rates.

that the favorable evidence of long-horizon predictive ability is present only over a two-year window of data vintages around those used in Mark (1995): most of the predictive ability eventually disappears after data revisions. Interestingly, they also find that the economic model performs better using real-time data than finally revised data. On the other hand, the empirical evidence shows that the predictive ability of Taylor-rule models is robust to using real-time data—see Molodtsova, Nikolsko-Rzhevskyy, and Papell (2011).

5.6 *What Have We Learned?*

Data transformations (such as detrending, filtering, and seasonal adjustment) may crucially affect predictive ability, and may explain why, for the same models, some researchers do find predictive ability while others do not. Cheung, Chinn, and Pascual (2005) and Alquist and Chinn (2008) find that the empirical evidence in favor of the single-equation ECM monetary model is much weaker or completely disappears after estimating the cointegrating parameters. The evidence in favor of the panel ECM model also relies on calibrated cointegration parameters, and it is unclear whether the findings would be robust to estimating the parameters; in addition, some panel ECM studies rely on data that have been previously demeaned over the full sample, which may create an unfair advantage for the economic model over the random walk. Another important factor that, for some fundamentals, may weaken predictive ability is using real-time, rather than realized, data; this is the case for monetary fundamentals but less of a concern for Taylor-rule fundamentals. For a given model and predictor, predictive ability also depends on the choice of the country. On the other hand, the frequency of the data and whether the realized or the forecasted fundamental is used do not seem to affect predictability: the monetary model's forecasts do not beat a random walk either way. However, there are exceptions.[60]

6. *Which Forecast Evaluation Methods To Choose?*

This section provides an overview of the methods that have been used in the literature to assess the predictive content of macroeconomic fundamentals for exchange rate forecasts as well as important issues to keep in mind when evaluating the predictive content. We also critically revisit the empirical findings in the previous sections in the light of these issues. Table 2, panel C, overviews the forecast evaluation methods used in selected papers.

6.1 *Choice of Benchmark Model*

The majority of studies compare the out-of-sample forecasting performance of the predictors with those of a random walk without drift (RW), as the latter has been shown to be the best predictor of exchange rates since Meese and Rogoff (1983a, 1983b)—see table 2, panel C. According to the random walk without drift, the best predictor of exchange rates tomorrow is the exchange rate today.[61] Thus, exchange rate changes are completely unpredictable:

$$E_t\, s_{t+h} - s_t = 0.$$

An alternative benchmark is the random walk with drift (RWWD), according to which exchange rate changes are predictable but

[60] Sometimes forecasts perform better using real-time forecasts of future fundamentals instead of actual future fundamentals, as in Faust, Rogers, and Wright (2003). Ferraro, Rogoff, and Rossi (2011) find that predictability is stronger using realized (as opposed to lagged) fundamentals.

[61] When not specified otherwise, random walk means the random walk without drift.

independent of other macroeconomic variables: $E_t\, s_{t+h} - s_t = \alpha \neq 0$.

It is important to note that the efficient market hypothesis does not imply that exchange rate changes should be unpredictable. That is, the Meese and Rogoff (1983a, 1983b) finding that the random walk provides the best prediction of exchange rates should not be interpreted as a validation of the efficient market hypothesis. The efficient market hypothesis means that bilateral exchange rate is the market's best guess of the relative, fundamental value of two currencies based on all available information at that time. The efficient market hypothesis does not mean that exchange rates (like any asset prices) are unrelated to economic fundamentals, nor that exchange rates should fluctuate randomly around their past values.

Consistently, across papers, the random walk is used as the benchmark. Typically, when the random walk with drift forecasts are reported, they are worse than those of the random walk without drift. This may explain some puzzling results in the literature. For example, among papers that study nonlinear models, Meese and Rose (1991) found more empirical evidence in favor of the nonlinear model, whereas Chinn (1991) and Chinn and Meese (1995) found the least. However, the benchmark in the former paper is the random walk with drift, whereas the benchmark in the latter two papers is the random walk without drift.

6.2 *Choice of Forecast Horizon*

Forecast evaluation also requires the choice of a forecast horizon, h. Forecast horizons typically considered in the literature range from short horizons (typically 1 month or 1 quarter ahead, depending on whether data are monthly or quarterly) to long horizons (horizons up to four to five years: for example, Mark (1995) considers forecast horizons up to 16 quarters, and Cheung, Chinn, and Pascual (2005) up to 20 quarters).

The choice of the forecast horizon is important since it is possible that the models' predictive ability may depend on it. Typically, the empirical evidence in favor of fundamentals' predictability appears at horizons that differ depending on the predictor. For example, for monetary fundamentals, most studies agree on lack of short-horizon predictability (i.e., Cheung, Chinn, and Pascual 2005); however, there is evidence of long horizon predictability at the three to four year horizon (Mark 1995). The predictability of Taylor-rule fundamentals is generally found at short horizons (Molodtsova and Papell 2009), whereas the predictability of net foreign assets models is found at both short and long horizons by some studies (Gourinchas and Rey 2007), and only at short horizons in some other studies (Alquist and Chinn 2008).

6.3 *Choice of Forecast Methodology*

Assessing the forecasting ability of a model requires several choices. First, which forecasting method should be used? The literature has been focusing mainly on rolling or recursive window forecasting schemes (see West 1996), where parameters are reestimated over time using a window of recent data—see section 2. Shorter estimation window sizes allow the parameter to adapt more quickly to structural changes; on the other hand, the parameter is less efficiently estimated in a smaller sample.[62] The choice of how to split the sample between in-sample and out-of-sample periods as well as the choice of the rolling window size varies across papers. For example, among papers using a rolling window scheme, Meese and Rogoff (1983a) use a window of 93 observations; Chinn (1991) uses 48; Qi and Wu (2003) use 216; Cheung, Chinn, and Pascual (2005) consider 42 and 59; van Dijk and Franses

[62] See e.g., Rossi (forthcoming).

(2003) use 128; and Clark and West (2006, 2007) and Molodtsova and Papell (2009) use 120 observations. See table 2, panel C, for additional details.

Rogoff and Stavrakeva (2008) and Rossi and Inoue (2012) examine the robustness of Taylor-rule and panel ECM monetary models and find that their performance changes substantially depending on the estimation window size. Thus, it is not the case that the predictive ability is present no matter which window size is used, as pointed out in Rogoff and Stavrakeva (2008); on the other hand, there are window sizes for which the predictive ability is significant, as discussed in Rossi and Inoue (2012). Regarding the net foreign asset model, Rogoff and Stavrakeva (2008) find that its performance is less sensitive, although still not entirely robust, to the choice of the window size.

6.4 *Choice of Evaluation Methods*

The forecast evaluation process requires two main choices: which loss function to use to evaluate the forecast, and which test statistic to use to assess significance. Regarding the choice of loss function, researchers typically evaluate models' out-of-sample forecasting performance according to their mean squared forecast error (MSFE) or root mean square forecast error (RMSFE), as in Meese and Rogoff (1983a, 1983b, 1988). The former is simply the average, over the out-of-sample period, of the squared forecast errors of a model; the latter is its square root—see section 2. Typically, researchers focus on either the RMSFE difference of competing forecasts or Theil's (1996) U statistics, which is the ratio of model RMSFEs to the RMSFEs of the benchmark model.[63] Note that this is only one of the available metrics/loss functions that can be used. Alternatively, researchers have used mean absolute errors ("MAE," as in Meese and Rogoff 1983a) and asymmetric loss functions (Ito 1990, West, Edison, and Cho 1993). Different loss functions give different weight to the deviations of the forecast from the target; for example, the MSFE gives equal weight to over- and under-predictions of the same magnitude.[64] Alternatively, the forecast evaluation exercise could target: (i) the direction of prediction, as in Engel (1994), Cheung, Chinn, and Pascual (2005) and Cerra and Saxena (2010); (ii) a utility-based measure, as in West, Edison, and Cho (1993), Rime, Sarno, and Sojli (2010), Abhyankar, Sarno, and Valente (2005) and Della Corte, Sarno, and Sestieri (2012); or (iii) the whole predictive density or interval forecasts, as in Diebold, Hahn, and Tay (1999), Inoue and Rossi (2008), and Wang and Wu (2009). The direction of change statistic calculates the proportion of forecasts that correctly predict the direction of change of the exchange rate.[65] As opposed to the RMSFE criterion, it focuses on whether the sign of the forecast change is correct, and, unlike the RMSFE, is not influenced by the distance between the forecast and the actual realization. In fact, it is theoretically possible that a model could forecast perfectly the direction of change in all periods and yet forecast worse than the random walk according to the RMSFE criterion; this may happen, for example, if the model consistently overpredicts the magnitude of the change. For example, Satchell and Timmermann (1995) theoretically prove that, whereas in linear models the probability of predicting the sign of a stochastic variable decreases when the MSFE increases, this monotonic relationship between the MSFE and the probability of correctly predicting the sign of the target variable does not need

[63] While there are other forms of Theil U statistic, this is the one commonly used in the exchange rate literature.

[64] See Elliott and Timmermann (2008) for a review of asymmetric loss functions.

[65] See Pesaran and Timmermann (1992) for a test for the direction of prediction.

to hold in general nonlinear models. Utility-based measures are typically interpreted as the transaction fee that a professional money manager can charge for providing estimates of the economic fundamental model to an investor that uses the benchmark model. The evaluation of the correct specification of density forecasts goes beyond point forecasts by evaluating the whole predictive density.[66]

The statistical significance of superior forecast performance is typically assessed via out-of-sample predictive ability tests or in-sample Granger causality tests. Both provide important insights and are used for different goals. The former are used for assessing whether predictors would have improved exchange rate predictions in forecasting environments that mimic as closely as possible the one faced by forecasters in practice, and have been used in Meese and Rogoff (1983a, 1983b). The latter are in-sample tests on whether the lagged predictor has significant explanatory power for exchange rates over the full sample, and have been used, for example, in Andersen et al. (2003).[67] It is important to keep in mind that the former is a much more challenging exercise than the latter: predictors that pass the latter test may still not have predictive ability in a truly out-of-sample forecasting exercise. Indeed, the Meese and Rogoff puzzle is the finding that, although fundamentals are significant predictors of exchange rates in-sample, their out-of-sample predictive ability is not superior to that of the random walk. A version of Granger-causality test robust to instability is Rossi (2005b); unlike the traditional Granger-causality tests, it detects predictive ability even if it appears only in a subsample, or if the predictive relationship changes over time. The latter test has been used, for example, in Chen, Rogoff, and Rossi (2010).

Traditional tests of out-of-sample predictive ability can be distinguished between absolute tests and relative tests of forecast evaluation. The former evaluate properties that optimal forecasts of a model should have, such as unbiasedness and uncorrelatedness.[68] The latter evaluate which, among competing models, forecasts the best, and include the tests proposed by Diebold and Mariano (1995), West (1996), Clark and McCracken's (2001) ENCNEW, Giacomini and White (2006), and Clark and West (2006, 2007), among others. The Meese and Rogoff puzzle concerns tests of relative predictive ability, since Meese and Rogoff (1983a, 1983b) compare the forecasting ability of fundamental-based exchange rate models with the random walk. While the tests for relative forecast performance developed in the literature are typically applied to MSFE differences between models, there is an important difference among them: on the one hand, West (1996), Clark and McCracken (2001), and Clark and West (2006, 2007) test out-of-sample whether the benchmark model is equivalent to the competing model *in population*; instead, Diebold and Mariano (1995) and Giacomini and White (2006) test

[66] It typically relies on the Probability Integral Transform (see Diebold, Hahn, and Tay 1999, and Corradi and Swanson 2006). See also Rossi and Sekhposyan (2013) for tests robust to instabilities.

[67] A different evaluation method is used in Engel and West (2005). They focus on evaluating whether exchange rates Granger-cause fundamentals in sample (rather than vice versa). The reason is, they show that if fundamentals have a unit root and the discount factor is near one, the net present value condition implies that exchange rates are near-random walks. Since the present value condition implies that exchange rates should predict fundamentals, they tested whether exchange rates Granger-cause fundamentals in-sample. While this is an implication of their framework, and therefore a clever way to provide indirect empirical evidence to substantiate their theory, it is nevertheless not a necessary condition, as exchange rates might Granger-cause future interest rates either because of the net present value condition or because interest rates are set by Central Banks to mitigate exchange rate fluctuations. Engel and West (2004) study the implications of a discount factor near unity for exchange rate volatility.

[68] See Mincer and Zarnowitz (1969), West and McCracken (1998), and Patton and Timmermann (2007a, 2007b).

whether two models' forecasting ability is the same. That is, the former test, using out-of-sample forecasts, whether, say, $\beta = 0$ in $E_t(s_{t+h} - s_t) = \beta f_t$; the latter test whether the forecasts of the fundamental model and that of the random walk are equivalent. Thus, the latter approach might be useful when the researcher is interested in evaluating forecasts, and the former when (s)he is interested in evaluating models in population. The difference between the two approaches is that, in nested models, the sample MSFE from the larger model is expected to be greater than that of the small model even when, in population, the two models have the same predictive ability, since the larger model introduces noise into its forecasts by estimating parameters that are useless in forecasting—see Clark and West (2006). Therefore, a finding that the small model has a smaller MSFE does not necessarily mean that the additional predictors present under the larger models are not useful for forecasting. The former tests take this into account in evaluating the relative predictive ability of competing models by recentering the MSFE differential in the test statistic by a term that accounts for parameter estimation error and hence, test whether, after adjusting for parameter estimation error, the larger model outperforms the benchmark—but parameter estimation error *is exactly one of the reasons* why there is a difference between in-sample fit and out-of-sample forecasts. Since the tests rely on different assumptions and null hypotheses, they have different power properties. See West (2006) and Diebold (2012) for insightful discussions. The majority of the papers in the exchange rate literature focus on the Diebold and Mariano (1995) and West (1996) tests (hereafter "DMW") or the Clark and West (2006, 2007) tests (hereafter "CW"). Table 2, panel C, provides an overview of which tests have been used in the literature.

The main conclusion that emerges from the literature is that the choice of the evaluation method matters, and a lot! For example, the use of different test statistics may explain the contradicting evidence on the empirical validity of UIRP. Typically, the majority of the studies that find lack of predictability for interest rate differentials either focus on RMSFEs or on the Diebold and Mariano (1995) test. Clark and West (2006) find that, based on the Diebold and Mariano (1995) and West (1996) tests, there is little evidence that UIRP beats the random walk; however, UIRP produces better forecasts than the random walk according to the Clark and West (2006) test. Similarly, using the Clark and West (2006) test, Alquist and Chinn (2008) conclude that UIRP can significantly outperform the random walk at long horizons.

The choice of the forecast evaluation method may also shed some light on the controversy over the empirical evidence on long horizon predictability of monetary fundamentals. On the one hand, Kilian (1999) and Berkowitz and Giorgianni (2001) dispute the finding of predictive ability of the monetary model at long horizons due to issues with the bootstrap implementation of the test and the fact that high persistence would distort inference. In particular, Berkowitz and Giorgianni (2001) and Berben and van Dijk (1998) show, via Monte Carlo simulations, that predictive ability measures of two completely unrelated time series could display large R-square and DMW statistics at long horizons if they are both highly persistent—a finding reminiscent of spurious regression. Kilian (1999) instead argues that, when bootstrapping the DMW statistic, it is important that the critical values of the statistic be calculated under the null hypothesis of the absence of a drift in the random walk; otherwise, the bootstrap model is not consistent with the model used as the benchmark. On the other hand, Rossi (2005c) shows that the high persistence in exchange rates and fundamentals biases the estimation of the economic models, but not the random walk: when this bias outweighs the benefits from exploiting economic predictors, the random walk forecasts better.[69] These findings show that the poor forecasting ability of economic models does not imply that the models are not a good description of the data.[70]

The choice of the evaluation metric may also be important in explaining differences in empirical results across studies. While Meese and Rogoff (1983a, 1983b) show that their negative finding is robust to using either mean squared forecast errors or mean absolute errors, several papers note that other metrics are more successful in finding out-of-sample evidence in favor of macroeconomic predictors: Engel (1994) and Cheung, Chinn, and Pascual (2005) find that the direction of change statistics provides more empirical evidence that models can outperform the random walk than MSFE comparisons; Abhyankar, Sarno, and Valente (2005) find similar results for utility-based measures.

6.5 *Choice of the Forecast Sample*

The sample utilized for out-of-sample forecast evaluation differs substantially across studies, not only because the studies have been performed at several points in time and differ in the overall samples they consider, but also because they differ in the choice of the window size used for estimation. Clearly, in the presence of instabilities, the models' performance could potentially be sensitive to the sample used for forecast evaluation.

Again, the literature is very clear on the following: the choice of the forecast sample matters enormously! In fact, one of the most interesting results in the literature concerning the robustness of results to the forecast period is the lack of robustness. For example, long-horizon predictability of the monetary model is very sensitive to the forecast period: Kilian (1999) and Groen (1999) find that the long-horizon predictability of the monetary model disappears once one extends Mark's (1995) sample. Cheung, Chinn, and Pascual (2005) consider two out-of-sample periods and note that the superior performance of a particular combination of monetary fundamental, model specification and country combination does not necessarily carry over from one out-of-sample period to the other. Rogoff and Stavrakeva (2008) note that neither the out-of-sample forecasting ability of panel VECM models nor that of Taylor rules are robust to the choice of the out-of-sample forecast period. Giacomini and Rossi (2010) note substantial changes over time in the predictive ability of Taylor-rule and UIRP fundamentals relative to the random walk.

The lack of robustness of results across sample periods is consistent with the possibility that the parameters might be unstable: in fact, Rossi (2006) shows that parameter instabilities are empirically important in exchange rate prediction. This suggests that it is crucial to consider evaluation methodologies that are robust to instabilities, such as Giacomini and Rossi's (2010) test, rather than traditional methods.[71] For example, using their proposed test, Giacomini and Rossi (2010) uncover a sharp worsening in the forecasting ability of UIRP in the late 1980s, to the point that traditional measures of predictive ability (that evaluate the performance of the model on average over the out-of-sample period) would overstate the recent predictive ability of the UIRP model. Rossi and Sekhposyan (2011) apply new methodologies to better understand why the economic models' performance is poor. They find that

69 This happens even if the economic model is the true data generating process.

70 A further limitation of traditional forecast evaluation tests is that they typically rely on stationarity assumptions, and are not robust to the presence of instabilities—see section 6.5.

71 See Rossi (2013) for an overview of forecast tests in the presence of instabilities.

lack of predictive content is the major explanation for the lack of short-term forecasting ability of the economic models, whereas instabilities play a role especially for medium term (one year ahead) forecasts.

6.6 *What Have We Learned?*

What we learn from this section is that the choice of the forecast evaluation method matters a lot. In particular, we note the following facts. The choice of the benchmark model is very important: choosing an inappropriate benchmark model overstates the empirical evidence in favor of the economic model's predictive ability; the random walk (without drift) is the toughest benchmark to beat and should be the one used in practice. The forecast horizon also matters a lot: typically, monetary fundamentals do not have short-horizon predictive ability (one month or one quarter), although the monetary fundamentals in an ECM model may forecast better than a random walk at long horizons (three to four years). In general, both the evaluation method as well as the forecast sample may potentially and substantially matter. For example, UIRP may outperform the random walk if certain tests are used (CW rather than DMW); in addition, its performance is unstable over time and worsened in the last decades. Rogoff and Stavrakeva (2008) find that the out-of-sample forecasting ability of the panel ECM model is not robust to either the choice of the forecast window or to the choice of the out-of-sample forecast period. To conclude, panel D in table 2 summarizes whether the predictors in the selected papers we consider are successful in forecasting exchange rates.

7. *Empirical Analysis*

To summarize, there is substantial disagreement in the literature regarding whether exchange rates are forecastable. The literature clearly reached a consensus on several "negative" stylized facts, namely the fact that the monetary and PPP fundamentals have no predictive ability at short horizons. There is also agreement on the fact that nonlinear models are the least successful ones. Finally, there is also agreement on the fact that, should monetary models have any predictive ability at long horizons, it only appears in single-equation ECM and panel ECM model specifications. On the other hand, there is substantial disagreement on whether monetary fundamentals do have predictive ability at long horizons and whether UIRP has predictive ability at short horizons. There is some evidence in favor of BMA models, although not strong. The only "positive" finding the literature reached a consensus on seems to be the fact that Taylor-rules and net foreign assets models have significant forecasting ability at short horizons.

In this last section, we undertake an empirical analysis to illustrate several of the different approaches, models, and methods used in the literature, and revisit the empirical findings using the most up-to-date sample, methods, and fundamentals we have available. We focus on univariate linear models with either traditional or newly proposed fundamentals, the single-equation ECM and panel ECM monetary models, as well as the BMA. Since nonlinear models fit well in-sample, but produce poor out-of-sample forecasts, we do not consider them in our empirical exercise; similarly, the most recent studies report lack of empirical support for time-varying parameter models, so we will not consider them either.

Given that the literature differs on the data transformations, we will focus on a unifying framework where the only data transformation is seasonal adjustment, which we perform consistently for all series. We use several of the methods reviewed in section 6 in the empirical analysis.[72]

[72] Data limitations prevent us from studying the importance of real-time versus revised data for the countries that we consider.

7.1 *Data Description*

We collect data on exchange rates (relative to the United States) and several economic fundamentals for a variety of countries: Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, New Zealand, Portugal, Spain, Sweden, Switzerland, the United Kingdom, and the United States.[73] For all countries, we collect monthly and quarterly data on overnight interest rates, three-month Treasury Bills, five-year Treasury Bonds, GDP/industrial production, CPI, and the money stock. We also collect data on the current account, the trade balance, public debt, and government deficit/surplus—the latter data are available only at the annual or quarterly frequency. Data are from the IMF database via Datastream, as well as Philip Lane's website (http://www.philiplane.org/EWN.html). Since countries' geographical definitions have changed over time (for example, after the start of the euro), the sample size differs across countries. Also, slightly different fundamentals are available, depending on the country (for example, the exact definition of money supply available might differ across countries, and the sample sizes for our measure of output for the same country differ depending on the frequency, since GDP is only available at the quarterly frequency, and industrial production is available at the monthly frequency). We selected the most homogeneous definitions we could find, while striving to collect data that have the longest available sample. Table 3 provides information on the available samples. A not-for-publication appendix provides additional details on the data, as well as their mnemonics and sources. The series are originally not seasonally adjusted, and seasonal adjustment was performed using one-sided moving averages with backward, equal weights.[74]

7.2 *Methodology*

We present empirical results using a few of the most used predictors and successful methodologies. Regarding the predictors, we consider interest rate differentials, price differentials, money and output differentials, Taylor-rule fundamentals, and measures of external imbalance, such as the current account and the trade balance.[75]

Regarding the models, first we consider the performance of selected traditional single-equation linear models (UIRP, eq.1.; PPP, eq. 2, and monetary, eq. 3) as well as Taylor-rule fundamentals (eq. 8) and portfolio balance models (eq. 6).[76] The monetary model is considered either as in eq. (3) with variables in first differences, or in the ECM form, eq. (12).[77] We will then turn to two representative multivariate models (the BMA and the panel VECM models). We will compare the models' forecasting performance at short (one month or one quarter) and long (four years) horizons with that of a random walk without drift using RMSFE comparisons. The models' parameters are recursively reestimated with a rolling window whose size is equal to half of the total sample size in the baseline analysis, although we investigate the

[73] While we collected data also for Argentina, Bolivia, Brazil, Chile, Colombia, Costa Rica, Hong Kong, Mexico, Peru, Russia, Singapore, Taiwan, Thailand, and Turkey, the sample sizes of either the exchange rate or the fundamentals were severely limited, or potentially severely affected by measurement error. These countries were discarded from the analysis.

[74] For quarterly data, the filter is $1/3 + 1/3L + 1/3L^2$; for monthly data it is $(1/12) + (1/12)L + \cdots + (1/12)L^{11}$. Empirical results based on seasonally unadjusted data are qualitatively similar.

[75] The analysis will focus on monthly data when possible, given that both monthly and quarterly data have the same span, but the former provides larger sample sizes.

[76] Unfortunately, net foreign assets data are available only at the annual frequency, so an analysis with the techniques currently used in this paper is not possible due to the small sample sizes.

[77] We report results for calibrated cointegration parameters: $\phi = 1$, as in Mark (1995)—see the discussion in section 4.

TABLE 3
AVAILABLE SAMPLE SIZES, BY COUNTRY AND MODEL

Models in monthly data

	Exchange rate differential			Interest rate differential			CPI differential			Money differential			Output differential		
Country	Start	End	Obs.	Start	End	Obs.	Start	End	Obs.	Start	End	Obs.	Start	End	Obs.
Australia	1957:02	2011:06	653	1969:08	2011:06	503	n.a.	n.a.	n.a.	1975:03	2011:06	436	1957:02	2007:09	608
Austria	1957:02	1998:12	503	1967:02	1998:12	383	1995:02	2011:06	197	1997:10	2011:06	165	1957:02	2011:05	652
Belgium	1957:02	1998:12	503	1957:02	1999:01	504	1957:02	2011:06	653	2002:02	2011:06	113	1957:02	2011:03	650
Canada	1957:02	2011:06	653	1975:02	2011:06	437	1957:02	2011:06	653	1975:04	2011:06	435	1957:02	2011:05	652
Denmark	1957:02	2011:06	653	1972:02	2011:06	469	1980:02	2011:06	377	1991:02	2011:06	245	1957:02	2011:05	652
Finland	1957:02	1998:12	503	1978:01	2011:06	402	1960:02	2011:06	617	1960:02	2011:06	617	1957:02	2011:06	653
France	1957:02	1998:12	503	1964:02	1999:03	418	1990:02	2011:06	257	1978:01	2011:06	402	1957:02	2011:05	652
Germany	1957:02	1998:12	503	1960:02	2011:06	617	1957:02	2011:06	653	1973:02	2011:06	461	1958:02	2011:05	640
Greece	1957:02	2000:12	527	1998:02	1999:10	021	1959:02	2011:06	629	1980:02	2011:06	377	1957:02	2011:05	652
Ireland	1957:02	1998:12	503	1972:04	2011:06	450	1969:02	2011:06	509	1999:02	2011:06	149	1976:02	2011:05	424
Italy	1957:02	1998:12	503	1971:02	2011:06	485	1957:02	2011:06	653	1980:02	2011:06	377	1957:02	2011:05	652
Japan	1957:02	2011:06	653	1957:02	2011:06	653	2000:02	2011:06	137	1960:02	2011:06	617	1957:02	2011:06	653
New Zealand	1957:02	2011:06	653	1985:02	2011:06	317	n.a.	n.a.	n.a.	1977:04	2011:06	411	1987:02	2007:12	251
Portugal	1957:02	1998:12	503	1983:02	2000:03	206	1960:02	2011:06	617	1997:10	2011:06	165	1957:02	2006:04	591
Spain	1957:02	1998:12	503	1974:02	2011:06	449	1957:02	2011:06	653	1997:10	2011:06	165	1961:02	2011:05	604
Sweden	1957:02	2011:06	653	1966:01	2011:06	546	1960:02	2011:06	617	1998:02	2011:06	161	1957:02	2011:05	652
Switzerland	1957:02	2011:06	653	1975:10	2011:06	429	1957:02	2011:06	653	1985:01	2011:06	318	1995:02	2007:12	155
U.K.	1957:02	2011:06	653	1972:02	2011:06	473	1988:02	2011:06	281	1986:10	2011:06	297	1957:02	2011:05	652

Models in Quarterly Data

	Int. Rate Differential			CPI Differential			TB Differential			Output Differential			CA Differential		
Country	Start	End	Obs.	Start	End	Obs.	Start	End	Obs.	Start	End	Obs.	Start	End	Obs.
Australia	1969:04	2011:02	167	1957:02	2011:02	217	1973:02	2010:04	151	1959:04	2011:02	207	1973:02	2010:04	151
Austria	1967:02	1998:04	127	1957:02	2011:02	217	1973:02	2011:01	152	1988:02	2011:01	092	1973:02	2011:01	152
Belgium	1957:02	1998:04	167	1957:02	2011:02	217	2002:02	2011:01	036	1980:02	2011:02	125	2002:02	2011:01	036
Canada	1975:02	2011:02	145	1957:02	2011:02	217	1973:02	2011:01	152	1961:02	2011:02	201	1973:02	2011:01	152
Denmark	1972:02	2011:02	155	1980:02	2011:02	125	1975:02	2011:01	144	1991:02	2011:02	081	1975:02	2011:01	144
Finland	1978:02	2011:02	133	1960:02	2011:02	205	1975:02	2011:01	144	1975:02	2011:02	145	1975:02	2011:01	144
France	1957:02	1999:01	168	1990:02	2011:02	085	1975:02	2010:04	143	1957:02	2011:02	217	1975:02	2010:04	143
Germany	1957:02	2011:02	217	1957:02	2011:02	217	1973:02	2011:01	152	1991:02	2011:02	081	1973:02	2011:01	152
Greece	1998:02	1999:03	006	1959:02	2011:02	209	1976:02	2011:01	135	2000:02	2011:01	044	1976:02	2011:01	135
Ireland	1973:02	2011:02	147	1969:02	2011:02	169	1981:02	2011:01	120	1997:02	2011:01	056	1981:02	2011:01	120
Italy	1971:02	2011:02	161	1957:02	2011:02	217	1973:02	2011:01	152	1981:02	2011:02	121	1973:02	2011:01	152
Japan	1957:02	2011:02	217	2000:02	2011:02	045	1977:02	2011:01	136	1980:02	2011:02	125	1977:02	2011:01	136
New Zealand	1985:02	2011:02	105	1957:02	2011:02	217	1980:02	2011:01	124	1987:03	2011:01	095	1980:02	2011:01	124
Portugal	1981:02	2000:01	076	1960:02	2011:02	205	1975:02	2011:01	144	1995:02	2011:02	065	1975:02	2011:01	144
Spain	1974:02	2011:02	149	1957:02	2011:02	217	1975:02	2011:01	144	1995:02	2011:02	065	1975:02	2011:01	144
Sweden	1966:02	2011:02	181	1960:02	2011:02	205	1975:02	2011:01	144	1993:02	2011:02	073	1975:02	2011:01	144
Switzerland	1976:01	2011:02	142	1957:02	2011:02	217	1999:02	2011:01	048	1980:02	2011:02	125	1999:02	2011:01	048
U.K.	1972:02	2011:02	157	1988:02	2011:02	093	1973:02	2011:01	152	1957:02	2011:02	217	1973:02	2011:01	152

Notes: The tables report the sample sizes available for each predictor and each country. "Start" denotes the start of the sample; "End" denotes the end of the sample; "Obs." denotes the total number of observations.

consequences of varying both the size of the window and the forecast sample. Since the literature review highlights the importance of the forecast sample, we formally investigate the robustness (or lack thereof) of the empirical findings to both the choice of the out-of-sample forecast period and the choice of the estimation window size by using the tests proposed by Giacomini and Rossi (2010) and Rossi and Inoue (2012), respectively.[78]

We will compare models' performance using both in-sample and out-of-sample tests. As discussed in the previous section, out-of-sample forecast tests may have different power properties, so in the empirical application, we shed some light on this issue by considering both the traditional DMW and the CW tests.

7.3 *Empirical Evidence*

Results are reported in tables 4 and 5. In greater detail, for each model that we consider, the first column reports the country whose nominal exchange rate growth we are forecasting (relative to the U.S. dollar). The second column (labeled "GC") reports p-values of Rossi's (2005b) Granger causality test robust to instabilities.[79] The third column reports the ratio of the root mean squared forecast error of the model relative to that of the random walk without drift (labeled "RMSFER"): values smaller than unity denote that the model forecasts better than the random walk benchmark. Finally, the last two columns assess the significance of the test for out-of-sample forecasting ability: the column labeled "DMW" reports p-values of the Diebold and Mariano (1995) and West (1996) test and the column labeled "CW" reports p-values of the Clark and West (2006) test.[80] The benchmark model in the latter two tests is the random walk without drift. "h" denotes the forecast horizon. All tests are implemented with Newey and West's (1987) heteroskedasticity and serial correlation robust covariance matrix.[81] Note that p-values lower than, say, 0.05, denote significance at the 5 percent level, which is the definition of significance we adopt in what follows when discussing the results. In all the tables, "n.a." denotes cases in which the sample size was too small and the test statistic is not reported.

7.3.1 *Traditional Predictors' Performance in Monthly Data*

Table 4, panels A–D report the performance of the traditional predictors. The panels show some in-sample predictive ability at short (one-month-ahead) horizons for UIRP and the monetary ECM models,[82] but very limited at long (four-years-ahead) horizons for any of the models.[83] The out-of-sample predictive ability is almost nonexistent; typically, models' MSFEs are larger than the random walk's,[84] and neither DMW nor CW find significant predictive ability. Why is it that our results for the monetary ECM

[78] Note that since the total sample size varies by country, the size of the rolling window as well as the out-of-sample forecast sample vary by country, too.

[79] Detailed empirical results based on traditional Granger causality tests are reported in the not-for-publication appendix, and their findings are summarized in this section.

[80] While the DMW test is typically used for nonnested models' forecast comparisons, in this article, we interpret its critical values following Giacomini and White (2006), who show that the DMW test can be used to compare forecasts of nested models provided that a rolling window is used for estimation.

[81] The truncation parameter is $T^{1/4}$, where T is the available sample size.

[82] In fact, seven countries are significant for UIRP, three for PPP, one for the monetary, and nine for the monetary ECM models. These results show that, for example, interest rates have been capable, at some point in time, to predict one-month-ahead exchange rates. According to the traditional Granger-causality test, reported in the not-for-publication appendix, traditional fundamentals have more limited predictive ability, significant only for a couple of countries for each model/predictor.

[83] PPP is significant for four countries; the other models/predictors perform much worse.

[84] MSFER is bigger than or equal to one.

TABLE 4
Relative Models' Forecasting Ability (Single Eq. Models)

Country	A. UIRP model GC	RMSFE	DMW	CW	B. PPP model GC	RMSFE	DMW	CW	C. Monetary model GC	RMSFE	DMW	CW
	$h = 1$ month				$h = 1$ month				$h = 1$ month			
Australia	0.44	1.00	0.52	0.58	n.a.	n.a.	n.a.	n.a.	0.06	1.00	0.51	0.27
Austria	0.10	1.01	0.53	0.48	0.27	1.07	0.74	0.99	n.a.	n.a.	n.a.	n.a.
Belgium	0.26	1.01	0.54	0.56	1.00	1.01	0.56	0.91	n.a.	n.a.	n.a.	n.a.
Canada	0.00	1.00	0.51	0.40	0.09	0.99	0.45	0.00	1.00	1.01	0.54	0.88
Denmark	1.00	1.01	0.56	0.95	0.11	1.00	0.52	0.55	0.34	1.01	0.56	0.58
Finland	0.63	1.02	0.57	0.86	0.50	1.00	0.53	0.76	0.38	1.01	0.55	0.72
France	0.78	1.02	0.54	0.75	0.42	1.02	0.59	0.88	0.72	1.01	0.55	0.73
Germany	0.00	1.01	0.53	0.47	0.82	1.00	0.52	0.57	0.37	1.01	0.53	0.34
Greece	0.00	1.00	0.51	n.a.	0.13	1.00	0.49	0.02	0.54	0.99	0.48	0.00
Ireland	0.00	1.03	0.54	0.76	0.00	1.01	0.54	0.58	n.a.	n.a.	n.a.	n.a.
Italy	0.28	1.02	0.55	0.80	0.00	1.01	0.55	0.76	0.08	1.00	0.51	0.28
Japan	0.05	1.01	0.54	0.81	0.47	1.01	0.51	0.31	0.26	1.01	0.52	0.25
New Zealand	1.00	1.01	0.53	0.53	n.a.	n.a.	n.a.	n.a.	0.01	1.00	0.50	0.32
Portugal	0.00	1.02	0.58	0.91	0.00	1.01	0.56	0.77	n.a.	n.a.	n.a.	n.a.
Spain	0.74	1.02	0.56	0.81	0.07	1.01	0.58	0.97	n.a.	n.a.	n.a.	n.a.
Sweden	0.00	1.04	0.53	0.88	0.19	1.00	0.50	0.30	0.17	1.04	0.62	0.98
Switzerland	0.45	1.01	0.55	0.84	0.36	1.00	0.50	0.38	0.78	1.01	0.58	0.91
U.K.	1.00	1.01	0.58	1.00	1.00	1.01	0.54	0.70	0.61	1.01	0.58	0.89
	$h = 4$ years				$h = 4$ years				$h = 4$ years			
Australia	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	0.21	1.00	0.52	0.29
Austria	0.53	1.00	0.53	0.67	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
Belgium	1.00	1.01	0.53	0.50	0.47	1.01	0.58	0.97	n.a.	n.a.	n.a.	n.a.
Canada	0.43	1.01	0.53	0.36	0.28	1.00	0.52	0.40	1.00	1.00	0.53	0.73
Denmark	0.71	1.00	0.52	0.43	0.72	1.00	0.51	0.44	1.00	1.02	0.57	0.70
Finland	0.31	1.01	0.55	0.78	0.83	1.00	0.55	0.84	0.55	1.01	0.55	0.50
France	1.00	1.02	0.57	0.76	0.53	1.04	0.61	0.78	0.21	1.02	0.55	0.73
Germany	0.82	1.01	0.54	0.60	0.04	1.01	0.54	0.69	0.22	1.01	0.54	0.53
Greece	0.77	1.01	0.53	0.49	0.22	1.00	0.49	0.03	0.43	1.00	0.49	0.04
Ireland	1.00	1.02	0.56	0.85	0.40	1.01	0.56	0.83	n.a.	n.a.	n.a.	n.a.
Italy	1.00	1.02	0.56	0.70	0.04	1.00	0.51	0.36	0.35	1.04	0.62	0.97
Japan	0.64	1.01	0.55	0.85	0.22	1.02	0.57	0.64	0.13	1.01	0.52	0.36
New Zealand	0.64	1.01	0.55	0.71	n.a.	n.a.	n.a.	n.a.	0.22	1.01	0.54	0.72
Portugal	1.00	1.02	0.58	0.84	0.00	1.01	0.53	0.47	n.a.	n.a.	n.a.	n.a.
Spain	0.70	1.02	0.55	0.61	0.05	1.00	0.53	0.63	n.a.	n.a.	n.a.	n.a.
Sweden	0.44	1.04	0.54	0.75	0.59	1.00	0.53	0.46	0.80	1.02	0.59	0.62
Switzerland	0.19	1.01	0.53	0.46	0.13	1.00	0.52	0.40	0.39	1.01	0.54	0.54
U.K.	0.24	1.01	0.57	0.92	0.63	1.00	0.50	0.21	0.56	1.00	0.54	0.67

(Continued)

TABLE 4
Relative Models' Forecasting Ability (Single Eq. Models) (*Continued*)

Country	D. Monet ECM model GC	RMSFER	DMW	CW	E. Taylor model GC	RMSFER	DMW	CW	F. Portfolio balance GC	RMSFER	DMW	CW
	$h = 1$ month				$h = 1$ month				$h = 1$ quarter			
Australia	0.00	1.01	0.55	0.89	n.a.	n.a.	n.a.	n.a.	0.66	1.06	0.56	0.31
Austria	n.a.	n.a.	n.a.	n.a.	0.00	0.98	0.54	0.14	1.00	1.06	0.61	0.77
Belgium	n.a.	n.a.	n.a.	n.a.	0.82	1.00	0.48	0.33	n.a.	n.a.	n.a.	n.a.
Canada	0.01	1.00	0.57	0.99	0.22	0.99	0.46	0.00	0.30	1.03	0.55	0.65
Denmark	0.05	1.00	0.52	0.50	0.35	1.00	0.52	0.28	0.00	1.04	0.59	0.72
Finland	0.39	1.00	0.51	0.35	0.48	1.00	0.48	0.15	0.00	1.08	0.64	0.84
France	0.02	1.01	0.54	0.50	0.66	1.02	0.54	0.60	0.00	1.27	0.70	0.77
Germany	0.00	1.00	0.54	0.76	0.51	0.99	0.45	0.03	0.01	1.09	0.62	0.85
Greece	0.14	1.00	0.52	0.53	0.00	0.99	0.47	0.00	n.a.	n.a.	n.a.	n.a.
Ireland	n.a.	n.a.	n.a.	n.a.	0.06	1.01	0.54	0.44	0.00	1.12	0.58	0.45
Italy	0.00	1.01	0.56	0.63	0.03	1.00	0.47	0.04	0.08	1.07	0.58	0.77
Japan	0.06	1.00	0.52	0.59	0.48	0.96	0.42	0.03	0.00	1.24	0.65	0.31
New Zealand	0.00	1.01	0.57	0.94	n.a.	n.a.	n.a.	n.a.	0.02	1.04	0.56	0.50
Portugal	n.a.	n.a.	n.a.	n.a.	0.01	1.01	0.53	0.25	0.00	1.15	0.63	0.76
Spain	n.a.	n.a.	n.a.	n.a.	0.02	1.00	0.49	0.08	0.00	1.04	0.55	0.44
Sweden	0.00	0.99	0.46	0.13	0.24	0.99	0.48	0.01	0.00	1.03	0.55	0.38
Switzerland	0.02	1.02	0.63	0.99	0.44	1.01	0.55	0.25	0.00	1.12	0.57	0.09
U.K.	0.20	1.01	0.55	0.61	1.00	1.00	0.49	0.10	0.20	1.03	0.57	0.29
	$h = 4$ years				$h = 4$ years				$h = 4$ years			
Australia	0.57	1.01	0.57	0.93	n.a.	n.a.	n.a.	n.a.	0.05	1.03	0.60	0.86
Austria	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	0.00	1.05	0.59	0.11
Belgium	n.a.	n.a.	n.a.	n.a.	0.89	1.00	0.51	0.49	n.a.	n.a.	n.a.	n.a.
Canada	0.50	1.00	0.51	0.38	0.12	0.99	0.48	0.00	0.00	1.03	0.57	0.65
Denmark	0.29	1.02	0.57	0.71	1.00	1.01	0.49	0.36	0.26	1.04	0.59	0.59
Finland	0.79	1.01	0.54	0.66	0.10	1.02	0.51	0.46	0.00	1.09	0.65	0.77
France	0.51	1.01	0.55	0.80	0.23	n.a.	n.a.	n.a.	0.00	1.21	0.65	n.a.
Germany	0.48	1.01	0.56	0.86	0.40	1.01	0.49	0.28	0.01	1.11	0.61	0.60
Greece	0.78	1.00	0.51	0.16	0.21	1.00	0.50	0.01	n.a.	n.a.	n.a.	n.a.
Ireland	n.a.	n.a.	n.a.	n.a.	0.04	1.03	0.57	0.45	0.00	1.11	0.71	0.52
Italy	0.31	1.03	0.59	0.80	0.02	1.01	0.50	0.33	0.00	1.06	0.56	0.42
Japan	0.39	1.01	0.53	0.80	0.00	n.a.	n.a.	n.a.	0.00	1.09	0.61	0.22
New Zealand	0.89	1.00	0.52	0.28	n.a.	n.a.	n.a.	n.a.	0.02	1.06	0.63	0.90
Portugal	n.a.	n.a.	n.a.	n.a.	0.00	1.12	0.55	0.20	0.00	1.06	0.59	0.28
Spain	n.a.	n.a.	n.a.	n.a.	0.00	1.00	0.45	0.05	0.00	1.06	0.58	0.41
Sweden	1.00	1.02	0.59	0.81	0.21	0.99	0.50	0.02	0.02	1.04	0.59	0.69
Switzerland	1.00	1.00	0.52	0.27	0.49	n.a.	0.63	0.17	0.00	1.04	0.60	0.60
U.K.	1.00	1.01	0.54	0.68	1.00	1.00	0.47	0.11	0.61	1.07	0.63	0.91

Notes: Tables 4 and 5 report p-values of the following tests: Rossi's (2005), Granger-causality robust ("GC"), Diebold and Mariano (1995) and West (1996) ("DMW"), and Clark and West (2006) ("CW"); the benchmark model in the latter two tests is the random walk without drift. "h" denotes the forecast horizon. "RMSFER" denotes the ratio of the root mean squared forecast error of the model relative to that of the random walk without drift: values smaller than unity denote that the model forecasts better than the random walk benchmark.

model are so different from those in the literature? We investigate this issue at the end of this section.

7.3.2 *Alternative Predictors' Performance*

Panel E in table 4 shows empirical evidence in favor of in-sample predictability of Taylor-rule fundamentals for several countries.[85] The CW test finds empirical evidence in favor of the Taylor-rule model only at short horizons: it is strongly significant for six countries and marginally for two others at the one-month-ahead horizon, and for four of the countries at the four-year horizon. Among the fundamentals that we consider, then, they are among the most successful out-of-sample at short horizons. Importantly, note, however, that the DMW test does not find predictive ability. The discrepancy between the CW and DMW tests emphasizes that the way we treat parameter estimation error matters: if we compare models and correct inference for the fact that the Taylor model estimates more parameters than the random walk, we conclude that the Taylor model, when evaluated "in population," has better predictive ability; however, if we evaluate its forecasts "at the actual estimated parameter values," their forecasting ability is not superior to that of the random walk.

Panel F in table 4 reports results for the model that includes real interest rates, the trade balance, and the current account as predictors (at the quarterly frequency). Interestingly, among the models we consider, this is one with the strongest in-sample predictive ability; however, its out-of-sample performance is extremely poor (possibly due to the large number of parameters to estimate).

7.3.3 *Multivariate Models*

Results for multivariate models are reported in table 5. Panel A in table 5 shows that the monetary panel model displays some in-sample predictive ability (at some point in time) and almost nonexistent out-of-sample forecasting ability at short horizons; interestingly, however, there is some evidence of out-of-sample forecasting ability at long horizons for four countries. Again, note the very different results for CW and DMW: once more, the latter never finds predictive ability. Panel B in table 5 shows instead that the BMA is not significantly better than the random walk benchmark in forecasting out-of-sample at any horizon and for any test statistics.

7.3.4 *Instabilities in Predictive Performance*

Our results are broadly consistent with those in the literature, although in some cases they differ. For example, we find less out-of-sample forecasting ability at long horizons than other papers in the literature for the monetary model in error-correction form (ECM). Two of the possible reasons why our results may differ from those reported by other papers are that either the out-of-sample period is different, or the window size is different, or both. Let us consider each of these two effects separately. We report results based on the Giacomini and Rossi (2010) and the Rossi and Inoue (2012) tests in figures 1–6.

Figures 1, 2, 5, and 6 plot Giacomini and Rossi's (2010) fluctuation test statistic (solid line) along with its critical value (dotted line) for selected countries, models, and forecast horizons.[86] The x-axis denotes time. The fluctuation test examines how the forecast performance changed over time: whether the out-of-sample predictive ability of the

[85] We consider the symmetric, homogenous Taylor rule, (eq. 8).

[86] The fluctuation test is implemented with $m = 1/3$ in Giacomini and Rossi's (2010) notation.

TABLE 5
Relative Models' Forecasting Ability (Multiple Eqs. Models)

Country	A. Monetary panel model			B. BMA model		
	RMSFER	DMW	CW	RMSFER	DMW	CW
			$h = 1$ month			
Australia	1.01	0.53	0.34	1.00	0.53	0.72
Austria	1.30	0.90	n.a.	n.a.	n.a.	n.a.
Belgium	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
Canada	1.01	0.53	0.63	1.00	0.53	0.83
Denmark	1.02	0.61	0.99	1.00	0.53	0.66
Finland	1.01	0.55	0.78	1.01	0.57	0.91
France	1.00	0.52	0.31	1.00	0.51	0.38
Germany	1.01	0.53	0.46	1.00	0.51	0.55
Greece	1.00	0.50	0.02	1.09	0.67	n.a.
Ireland	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
Italy	1.00	0.51	0.29	1.00	0.53	0.59
Japan	1.01	0.53	0.62	1.00	0.49	0.21
New Zealand	1.01	0.54	0.44	1.00	0.54	0.78
Portugal	1.13	0.71	n.a.	n.a.	n.a.	n.a.
Spain	1.01	0.56	n.a.	n.a.	n.a.	n.a.
Sweden	1.00	0.54	0.69	1.01	0.57	0.89
Switzerland	1.03	0.61	0.97	1.00	0.52	0.56
U.K.	1.01	0.55	0.75	1.00	0.52	0.53
			$h = 4$ years			
Australia	1.35	0.70	0.31	1.00	0.48	0.18
Austria	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
Belgium	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
Canada	1.34	0.70	1.00	1.00	0.53	0.84
Denmark	1.49	0.76	1.00	1.01	0.55	0.83
Finland	1.84	0.80	0.00	1.01	0.55	0.87
France	1.72	0.78	1.00	1.00	0.54	0.79
Germany	1.75	0.76	1.00	1.00	0.52	0.50
Greece	1.14	0.60	0.00	n.a.	n.a.	n.a.
Ireland	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
Italy	1.11	0.55	0.00	1.00	0.54	0.73
Japan	1.79	0.74	0.50	1.00	0.52	0.59
New Zealand	1.52	0.69	1.00	1.00	0.49	0.27
Portugal	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
Spain	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
Sweden	1.10	0.61	0.02	1.00	0.49	0.21
Switzerland	1.27	0.77	0.87	1.00	0.49	0.20
U.K.	1.60	0.82	1.00	1.00	0.48	0.16

Notes: The tables report p-values of the following tests: Diebold and Mariano (1995) and West (1996) ("DMW"), and Clark and West (2006)("CW"); the benchmark model in the latter two tests is the random walk without drift. "h" denotes the forecast horizon. "RMSFER" denotes the ratio of the root mean squared forecast error of the model relative to that of the random walk without drift: values smaller than unity denote that the model forecasts better than the random walk benchmark.

traditional predictors ever showed up in the data, and, if so, when. For example, in figure 1, the fluctuation test (solid line) for Canada is never above its critical value (dotted line), thus the predictor never displays significant forecasting ability; on the other hand, the fluctuation test does detect predictive ability for France in the late 1990s, Japan in mid-2000, and the United Kingdom in 2009 (see figure 2). Overall, figure 1 shows that monetary fundamentals have occasional and very short-lived predictive ability at the one-month horizon for some countries at some point in time. Figure 2 shows similar, occasional predictive ability at long horizons.[87]

A second reason the results may differ is due to the use of a different estimation window size.[88] We study the behavior of the predictive ability as a function of the window size in figures 3 and 4. The figures plot Rossi and Inoue's (2012) test statistic (solid line) along with its critical value (horizontal line) for selected countries, models, and forecast horizons. The x-axis denotes the window size, R.[89] For example, figure 3 reports the DMW test calculated in rolling windows over the out-of-sample period for various window sizes R, reported on the x-axis. When the DMW test (solid line) is above Rossi and Inoue's (2012) critical values (dotted line), this signals predictive ability for that window size. Overall, figures 3 and 4 show that indeed the window size strongly affects predictability for some countries.[90]

However, note that the out-of-sample period changes as we change the estimation window size: a larger estimation window size implies that a larger proportion of the sample is used for estimation and a smaller proportion is used for out-of-sample forecast evaluation. To analyze how the choice of the estimation window size and the out-of-sample period interact with each other, figures 5 and 6 report the fluctuation test for various window sizes for Germany and Japan, respectively. Very interesting findings emerge from the figures. Figure 5 shows that only small estimation window sizes can detect forecasting ability of monetary fundamentals in Germany, and their predictive power is concentrated in the late 1980s; figure 6 shows instead that only large estimation window sizes can detect predictability of monetary fundamentals in Japan, and that the predictability emerges in the late 2000s.

Clearly, for some window sizes, it is possible to find some evidence of predictive ability for monetary fundamentals, thus confirming previous results that have been reported in the literature for selected window sizes and/or out-of-sample evaluation periods. At the same time, our results highlight the lack of robustness of these analyses to small changes in the procedures for forecast estimation and evaluation.

7.3.5 *Summary*

To summarize our results, most traditional predictors show in-sample forecasting ability, but none of them shows strong out-of-sample forecasting ability across all countries and tests. Turning to the most recent Taylor-rule predictors, they are among the most successful fundamentals out-of-sample at short horizons. Exploiting

[87] See Giacomini and Rossi (2010) for time variation in Taylor-rule fundamentals.

[88] In our exercise, we choose a rolling window with a size equal to half of the total sample size: while the fact that we select a window size equal to half of the total sample size is similar to what other papers have done, our total sample is still longer, and therefore, our results may differ from those in the literature; furthermore, the sample is different, too.

[89] We implement the sup-type version of Rossi and Inoue's (2012) test.

[90] Figure 3 shows predictability at the one-month-ahead horizon for Switzerland, Japan, France, and the United Kingdom: in the former, predictability is enhanced by a large window size, and in the latter two, it is the opposite. Figure 4 shows similar results for 48-month-ahead forecasts for Switzerland and Japan.

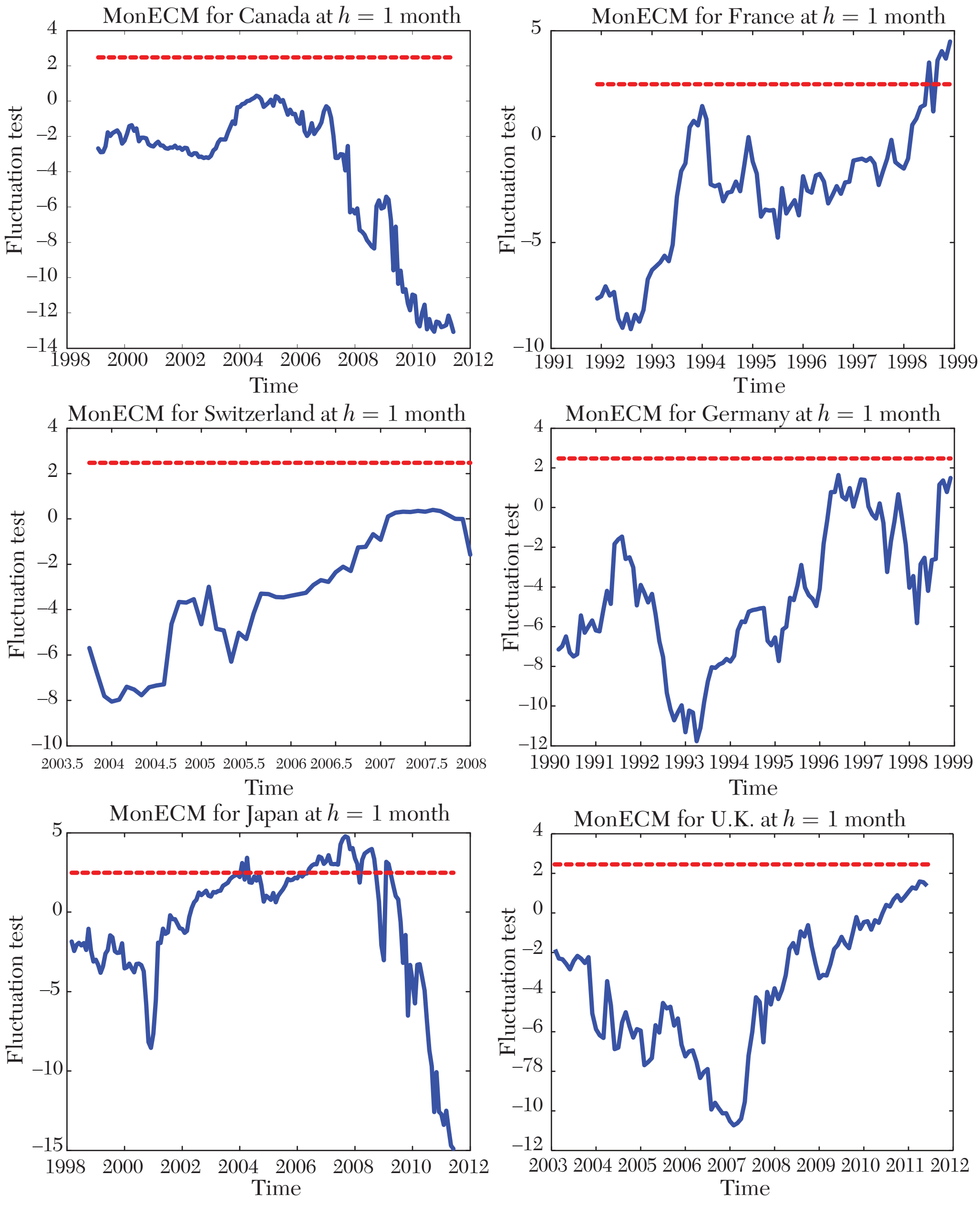

Figure 1. Fluctuation Test (Monetary-ECM Model, Short-Horizon)

Notes: The figures plot Giacomini and Rossi's (2010) fluctuation test statistic (solid line) along with its critical value (dotted line) for selected countries, models and forecast horizons. The x-axis denotes time. The window size used for parameter estimation, R, equals half of the total sample size. The window size used to smooth out-of-sample predictive ability in the fluctuation test is a third of the total number of forecasts.

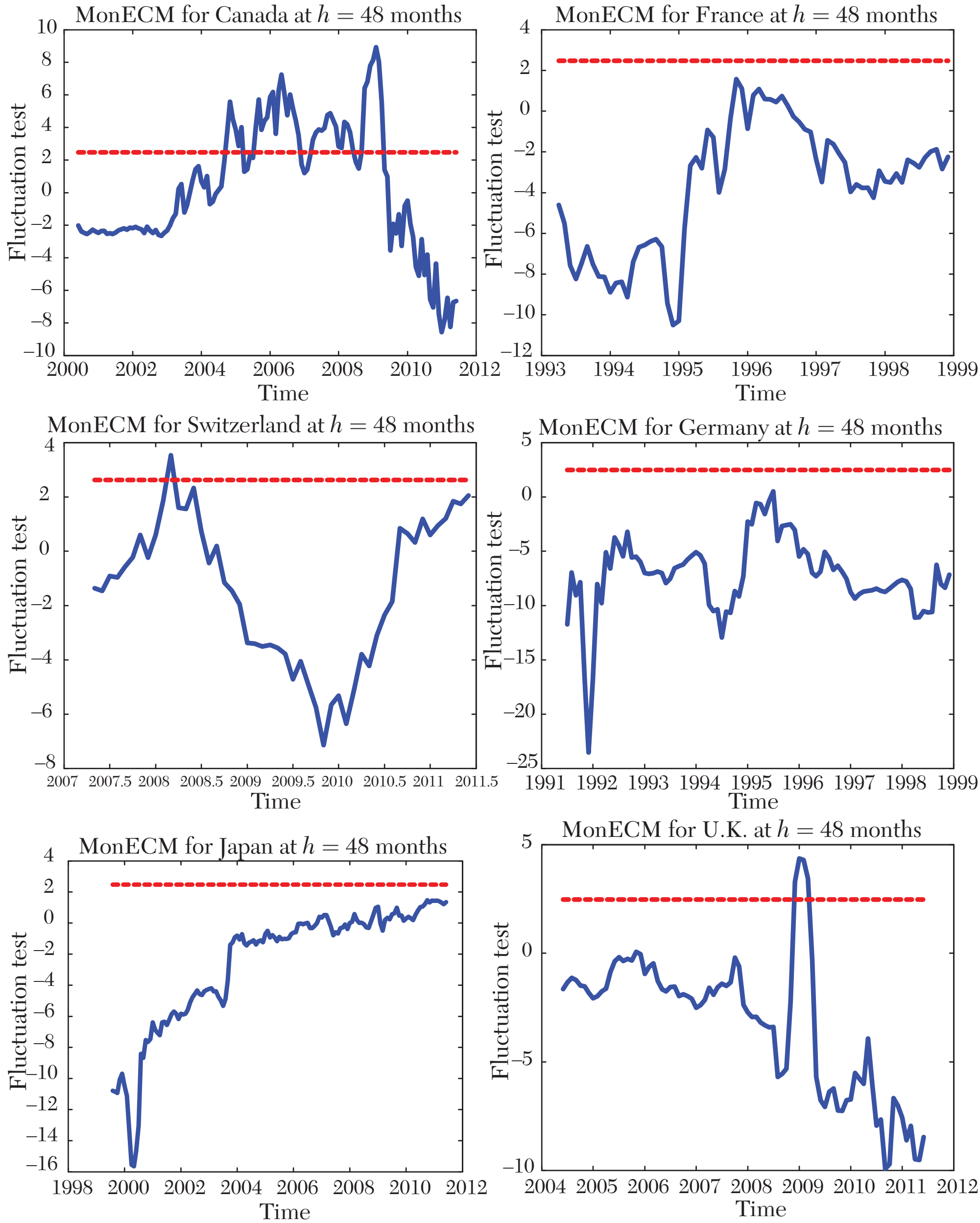

Figure 2. Fluctuation Test (Monetary-ECM Model, Long-Horizon)

Notes: The figures plot Giacomini and Rossi's (2010) fluctuation test statistic (solid line) along with its critical value (dotted line) for selected countries, models and forecast horizons. The x-axis denotes time. The window size used for parameter estimation, R, equals half of the total sample size. The window size used to smooth out-of-sample predictive ability in the fluctuation test is a third of the total number of forecasts.

information on fundamentals using panels does not seem to help at short horizons, although it provides forecast improvements over the random walk at long horizons for a few countries. Figures 7 and 8 summarize the empirical findings. Figure 7 is a scatterplot of the p-values of Clark and West's (2006) test at short horizons (reported on the x-axis) and at long horizons (reported on the y-axis) for several predictors, denoted by different markers. Each point in the figure corresponds to a country/predictor/model combination. p-values smaller than 0.05 denote statistical significance at the 5 percent level. The figure shows that, for several countries, Taylor-rule fundamentals are concentrated in the left, mid-to-upper corner of the figure; therefore, they are significant predictors at short horizons but not at long horizons. Conversely, the monetary panel model shows up on the right side of the figure; in some cases in the lower bottom corner, and in several other cases, in the top right corner. Therefore, the monetary panel model is significant at long horizons (but not at short horizons) for some countries, and is not significant at any horizon for several other countries. Figure 8 does the same for Rossi's (2005b) robust Granger causality test. It shows that Taylor-rule and monetary panel models perform the best in-sample (the former at both horizons, the latter at short horizons only); however, comparing figures 7 and 8 clearly shows that only the in-sample predictive ability of Taylor rules survives out-of-sample.

Only when we allow the predictability to be varying over time using the fluctuation test, we see a few episodes when the economic predictors' forecasting ability was stronger than that of the random walk benchmark even in traditional predictors, although they remain sporadic and short-lived in most countries. So, forecasting ability, when present, is an occasional and short-lived phenomenon.

8. *Conclusions*

To conclude, our analysis of the literature review has uncovered several stylized facts, which lead to five main conclusions.

First, a consensus in the literature is that the Taylor rule and net foreign assets fundamentals have more out-of-sample predictive content than traditional fundamentals (such as interest rate, inflation, output, and money differentials); in fact, monetary fundamentals at very long horizons and interest rate differentials at short horizons display forecasting ability according to some papers, but not others. However, the disagreement in the literature is in the extent to which the former can explain the Meese and Rogoff puzzle.

Second, overall, among the model specifications considered in the literature, the most successful are linear specifications. Among them, the single-equation ECM and the panel ECM models are the most successful at long horizons, although there is disagreement among researchers about the degree of robustness of the results. Typically, but not always, for single-equation linear models, the predictor choice matters more than whether the researcher uses contemporaneous, realized or lagged fundamentals. Among the multivariate models, the most successful specification is the panel ECM.

Third, data transformations (such as detrending, filtering and seasonal adjustment) may substantially affect predictive ability, and may explain some differences in results across studies. For example, the forecasting ability of the monetary model at long horizons is much weaker or completely disappears after estimating the cointegration parameters. Another important factor that, for some fundamentals, may affect predictive ability, is using realized or real-time data.[91] For a given model and predictor, predictive

[91] This is a concern for monetary fundamentals and less of a concern for Taylor-rule fundamentals.

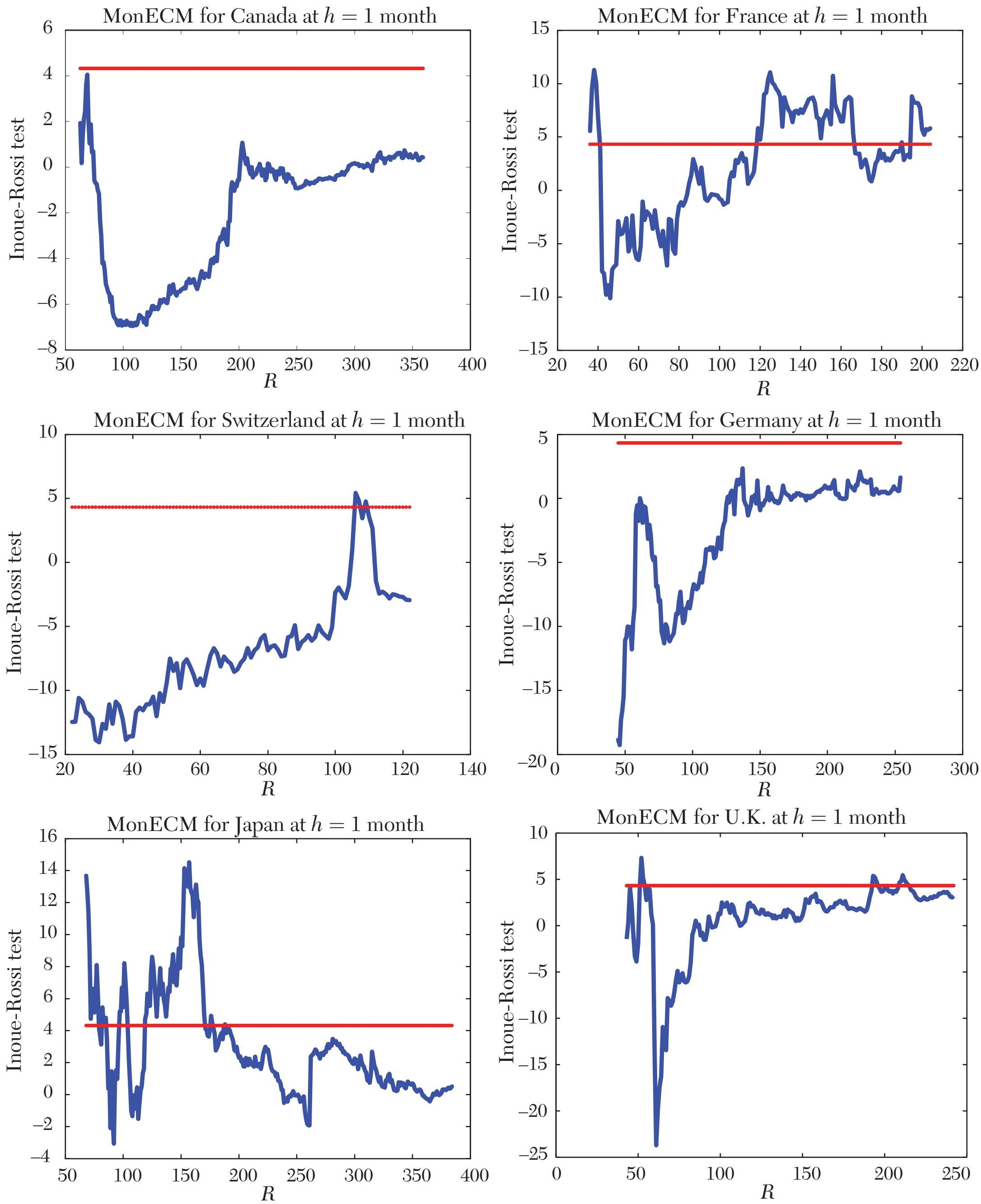

Figure 3. Robustness to Window Size (Monetary-ECM Model, Short-Horizon)

Notes: The figures plot Rossi and Inoue's (2012) sup-test statistic (solid line) along with its critial value (dotted line) for selected countries, models and forecast horizons. The x-axis denotes the window size *R*.

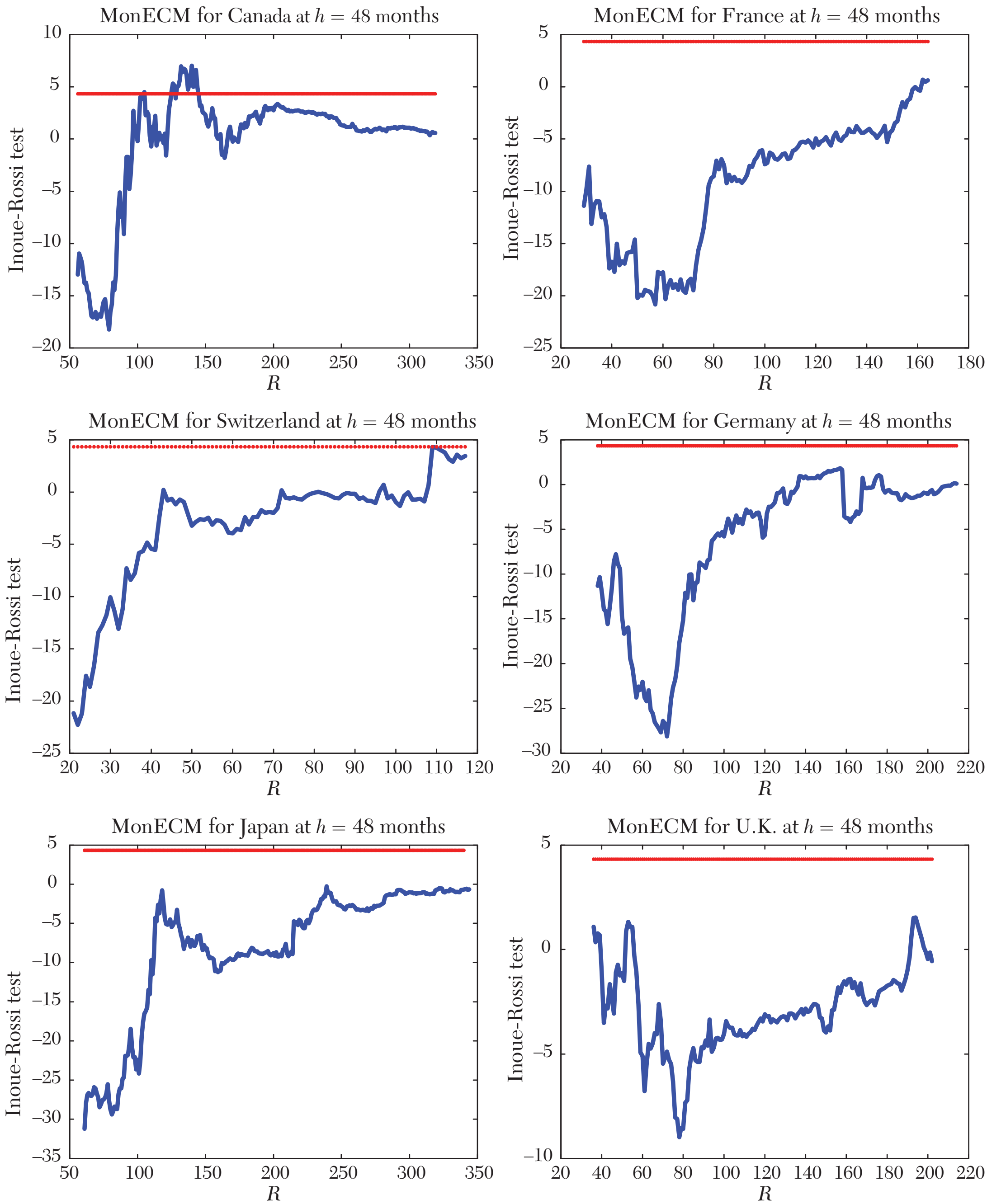

Figure 4. Robustness to Window Size (Monetary-ECM Model, Long-Horizon)

Notes: The figures plot Rossi and Inoue's (2012) sup-test statistic (solid line) along with its critial value (dotted line) for selected countries, models and forecast horizons. The x-axis denotes the window size R.

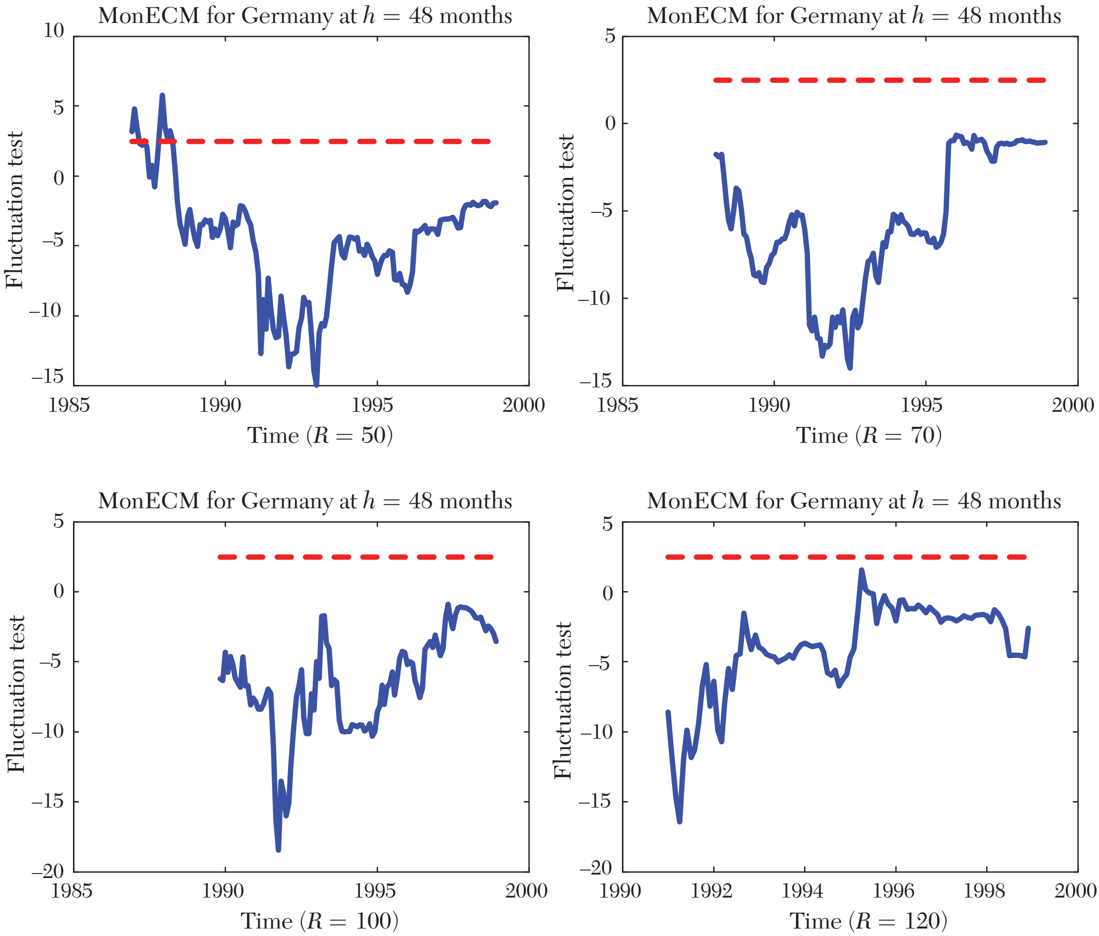

Figure 5. Interaction between Window Size and Out-of-Sample Period—Germany

Notes: The figures plot Giacomini and Rossi's (2010) Fluctuation test statistic (solid line) along with its critical value (dotted line) for selected countries, models and forecast horizons. The x-axis denotes time. R denotes the window size used for parameter estimation. The window size used to smooth out-of-sample predictive ability in the fluctuation test is a third of the total number of forecasts.

ability seems also to depend on the choice of the country; on the other hand, with few exceptions, the frequency of the data and whether the realized or the forecasted fundamental is used do not seem to affect predictive ability.

Fourth, the choice of the benchmark, horizon, sample period, and forecast evaluation method matters a lot. In particular, choosing an inappropriate benchmark model may overstate the empirical evidence in favor of the economic model's predictive ability:

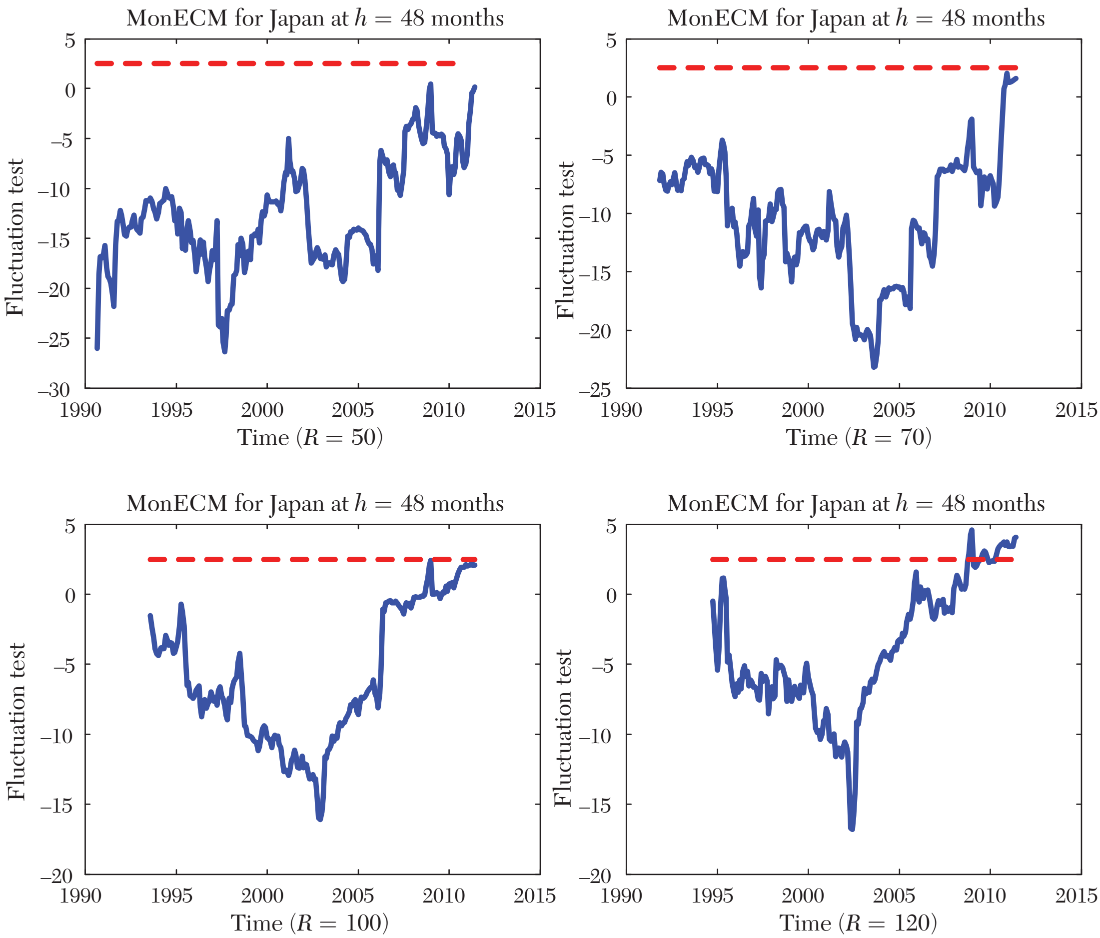

Figure 6. Interaction between Window Size and Out-of-Sample Period—Japan

Notes: The figures plot Giacomini and Rossi's (2010) fluctuation test statistic (solid line) along with its critical value (dotted line) for selected countries, models and forecast horizons. The x-axis denotes time. R denotes the window size used for parameter estimation. The window size used to smooth out-of-sample predictive ability in the fluctuation test is a third of the total number of forecasts.

the random walk without drift is the toughest benchmark to beat. Empirical results depend on the forecast horizon,[92] the choice of the evaluation method,[93] and the forecast

[92] For example, the literature agrees that monetary fundamentals do not have predictive ability over short horizons, but disagrees on whether they might have over long horizons.

[93] For example, whether interest rate differentials outperform the random walk may depend on the test that is used in practice.

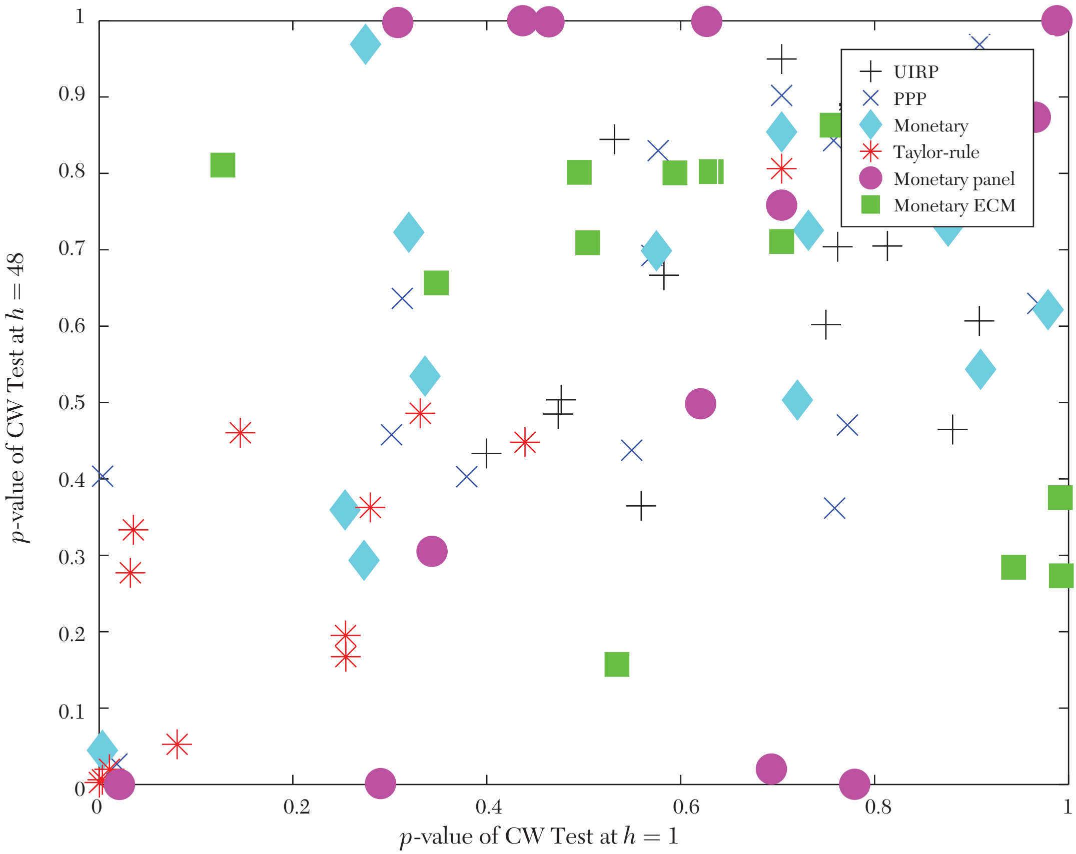

Figure 7. Out-of-Sample Predictive Ability of Economic Models

Notes: The figure is a scatterplot of the p-values of Clark and West's (2006) test at short horizons (on the x-axis) and at long horizons (on the y-axis) for several predictors, denoted by different markers, across multiple countries (each point denotes a country). p-values smaller than 0.05 denote statistical significance at the 5 percent level.

sample, as the performance of predictors is typically unstable over time.[94]

Fifth, on the one hand, the empirical analysis confirms several of the findings in the literature: several predictors display in-sample predictive ability for future exchange rates; however, only few predictors display out-of-sample forecasting ability: Taylor rules at short horizons for several countries, and monetary panel models at long horizons for some countries. *On the other hand, our literature review and empirical evidence suggest slightly less out-of-sample predictive ability* in favor of some of the models and predictors used in the literature.[95] The reason is

[94] For example, the out-of-sample forecasting ability of the panel ECM model is not robust to either the choice of the forecast window or the forecast period.

[95] For example, monetary fundamentals at long horizons.

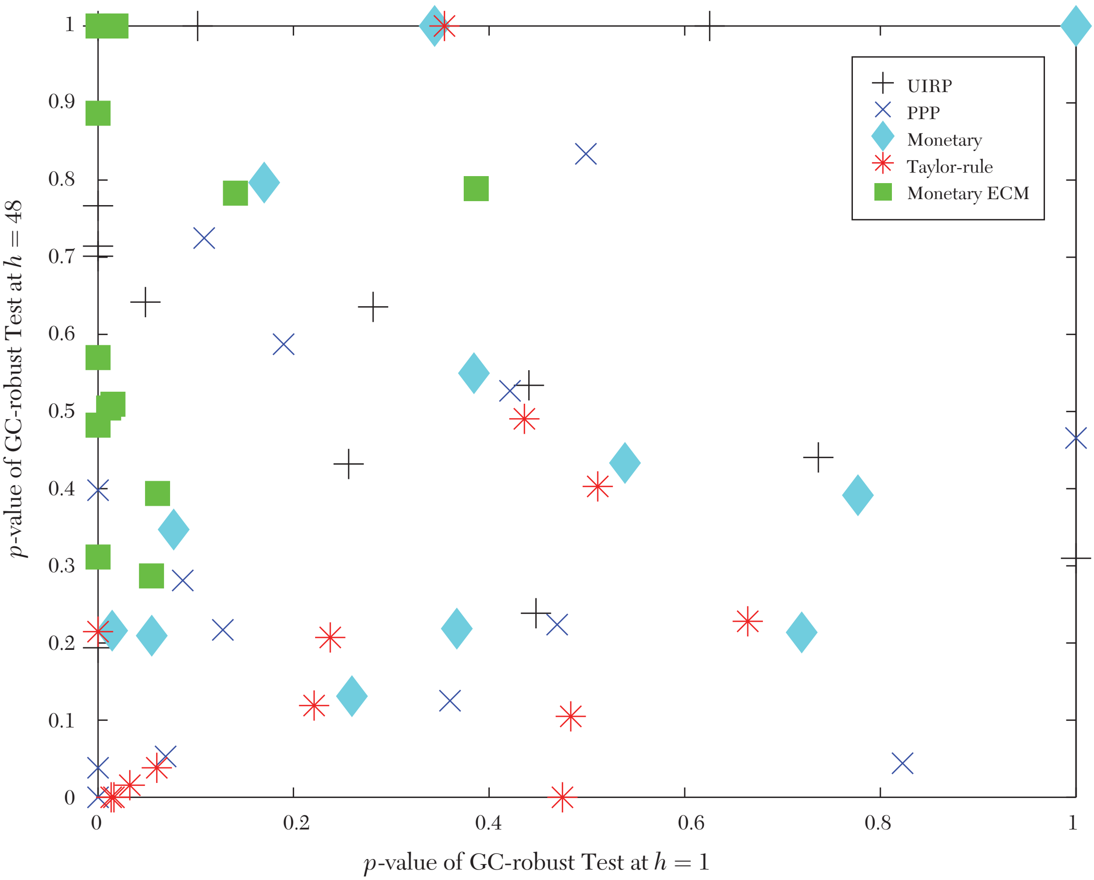

Figure 8. In-Sample Predictive Ability of Economic Models

Notes: The figure is a scatterplot of the p-values of Rossi's (2005b) robust GC test at short horizons (on the x-axis) and at long horizons (on the y-axis) for several predictors, denoted by different markers, across multiple countries (each point denotes a country). p-values smaller than 0.05 denote statistical significance at the 5 percent level.

that *there are substantial instabilities in the models' forecasting performance*: the predictive power of fundamentals varies not only across countries, models and predictors, but also over time periods; it may appear in some periods and disappear in others.

Overall, although some predictors (Taylor-rule fundamentals and net foreign assets) do exhibit some predictive ability at short horizons, and others (monetary fundamentals, especially in panel models) reveal some predictive ability at long horizons, none of the predictors, models, or tests systematically find empirical support for superior exchange rate forecasting ability of a predictor for all models, countries and time periods: typically, when predictability appears, it does so occasionally for some countries and for short periods of time. Thus, Meese and Rogoff's (1983a, 1983b) finding does not seem to be entirely and convincingly overturned. We also find another interesting puzzle: the predictive ability of the fundamentals is time-varying and occasional, yet existing time-varying parameter models are not successful in capturing it. Our findings lead to new challenges: why does the predictability of exchange rate models change over time? Is it possible to design ways to exploit instabilities and improve exchange rates' forecasts? An answer to these questions would require an answer to another important question: why are exchange rates poorly forecasted by economic models? In their papers, Meese and Rogoff (1983a, 1983b) conjectured that sampling error, model misspecification, and instabilities could potentially explain the poor forecasting performance of the economic models. Insights on the latter may be particularly relevant to understand and resolve the Meese and Rogoff puzzle.

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